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## A large discourse concerning algebra: John Wallis's 1685 *Treatise of algebra*

### Thesis

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A large discourse concerning algebra:

John Wallis's 1685

*Treatise of algebra*

A thesis submitted by

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for the degree of

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## Abstract

*A treatise of algebra historical and practical* (London 1685) by John Wallis (1616-1703) was the first full length history of algebra. In four hundred pages Wallis explored the development of algebra from its appearances in Classical, Islamic and medieval cultures to the modern forms that had evolved by the end of the seventeenth century. Wallis dwelt especially on the work of his countrymen and contemporaries, Oughtred, Harriot, Pell, Brouncker and Newton, and on his own contribution to the emergence of algebra as the common language of mathematics.

This thesis explores why and how *A treatise of algebra* was written, and the sources Wallis used. It begins by analysing Wallis's account of mathematical learning in medieval England, never previously investigated. In his researches on the origins and spread of the numeral system Wallis was at his best as a historian, and initiated many modern historiographical techniques. His summary of algebra in Renaissance Europe was less detailed, but for Wallis this part of the story set the scene for the English flowering that was to be his main theme.

The influence of Oughtred's *Clavis* on Wallis and his contemporaries, and Wallis's efforts to promote the book, are explored in detail. Wallis's controversial account of Harriot's algebra is also examined and it is argued that it was better founded than has sometimes been supposed and that Wallis had direct access to Harriot's algebra through Pell. Many other chapters of *A treatise of algebra* contain mathematics that can be linked or traced to Pell, a hitherto unsuspected secret of the book.

The later chapters of the thesis, like the final part of *A treatise of algebra*, explore Wallis's *Arithmetica infinitorum* and the work which arose from it up to Newton's foundation of modern analysis, and include a discussion of Brouncker's treatment of the number challenges set by Fermat. The thesis ends with a summary of contemporary and later reactions to *A treatise of algebra* and an assessment of Wallis's view of algebra and its history.

## Acknowledgments

It is customary to end pieces such as this with thanks to one's wife; I must begin with gratitude to my husband, a reader of book reviews, who thought that *The history of mathematics*, edited by Fauvel and Gray, might interest me. Little did he know what he was setting in motion, but his support over the succeeding years has allowed me to undertake and complete this thesis in considerably less time than it might otherwise have taken.

Next I owe a double debt of thanks to June Barrow-Green, my tutor on the Open University course *Topics in the history of mathematics*, for suggesting that I should begin this research and for the warm and positive interest she has taken in its progress ever since. I have been funded for three years by an Open University Studentship in the Faculty of Mathematics and Computing.

I could not have asked for a better supervisor than John Fauvel. He has allowed me a wonderful freedom to forge my own path while at the same time responding to every draft and every query with prompt, helpful and stimulating suggestions. Above all, without ever appearing to exert the least pressure, he has little by little raised my standards, my expectations and my confidence, for all of which, and much more, I am deeply grateful.

David Fowler and Tom Whiteside have given me moral and mathematical support from beginning to end. In addition I have received generous help from Philip Beeley, Stephen Clucas, Ivor Grattan-Guinness, Jan van Maanen, George Molland, Peter Neumann, John North, Christoph Scriba and Muriel Seltman.

The research would have been impossible without the aid of librarians and archivists in Oxford, Milton Keynes, Northampton, London, Hamburg and Gröningen. In particular I would like to thank the counter staff of Duke Humfrey's Library, Oxford, who have helped me to find my way amongst the rich but sometimes buried treasures of the Bodleian Library and who have supplied me with more volumes than I or they would care to count.

Pursuing research as an Open University student is a slightly unusual experience which makes its own demands, and I thank especially David Clover and Liz Scarna for so efficiently setting up and operating the technology that



made it possible for me to work eighty miles from a lending library or a supervisor. I am also grateful to colleagues in the Department of Pure Mathematics at the Open University and to my many friends in the British Society for the History of Mathematics for providing me with the much valued company of other mathematicians and historians. In addition I have greatly appreciated the many friends and neighbours closer to home who admit to having no interest at all in algebra or its history but who have nevertheless given me their warm personal support. I mention here especially Louanne, for her hospitality in Oxford, and Christine whose company so often restored me on my days off and who never complained as those days became increasingly rare.

Last, but perhaps most, I thank my children whose enthusiasm for this project when it was first conceived encouraged me to begin, and who have regarded it ever since with a mixture of gentle ridicule and immense pride. Their unerring faith in my ability to complete it sustained me more than they knew, and the finished work is a gift from me to them.

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The material of Chapter 4 has appeared in *Annals of Science* **57** (2000), 27-60, of Chapter 5 in *Archive for the History of the Exact Sciences* **54** (2000), 455-497, and of Chapter 8 in *Notes and Records of the Royal Society* **54** (2000), 293-331. These papers have been slightly modified and updated for inclusion in this thesis.

## Chapter 1

### Introduction: ‘a large discourse concerning algebra’

In April 1677 John Wallis, Savilian Professor of Geometry at Oxford, deposited some of his mathematical papers with John Collins of the Royal Society, as he and other mathematicians were accustomed to do while awaiting a suitable opportunity to publish.<sup>1</sup> By far the longest of Wallis’s assorted treatises was ‘a large discourse concerning algebra’. Eight years later, with several other pieces added in the meantime, the discourse was published as *A treatise of algebra both historical and practical, shewing the original, progress, and advancement thereof, from time to time; and by what steps it hath attained to the heighth at which now it is*. The book was not just the first history of algebra in English, but an invaluable compendium of work by seventeenth-century English mathematicians, both well known and obscure. An exploration in depth of its contents, the subject of this thesis, gives rise to a new picture of the evolution of algebra in its crucial formative period, especially in the seventeenth century and especially in England. The present thesis is therefore a new ‘discourse concerning algebra’ and a reappraisal of the work of some of the mathematicians who contributed to the development of a discipline and language without which modern mathematics and science would be, literally, unthinkable.

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<sup>1</sup> Wallis to Collins April 1677, Rigaud 1841, II, 606-607.

## **John Wallis (1616-1703)<sup>2</sup>**

John Wallis was born on 23 November 1616 at Ashford in Kent where his father was the incumbent. He was just six years old when his father died. When he was nine, his mother sent him away from Ashford, then afflicted by plague, to a tutor in Tenterden about twelve miles away, and he remained there until, at the age of fourteen, he spent a year at Felsted school in Essex. As was usual at the time, such an education gave him a thorough grounding in Latin (which he wrote as easily as English) and also in Greek and some Hebrew, but he learned elementary mathematics only from the textbooks and instruction of a younger brother who was preparing to go into trade.<sup>3</sup> From then on, during his years at Emmanuel College, Cambridge, (1632-1640) he pursued mathematics as 'a pleasing Diversion'.<sup>4</sup> In his autobiography, written sixty years later, he said that 'Mathematicks, (at that time, with us) were scarce looked upon as Academical Studies',<sup>5</sup> but he may have moved in relatively limited circles, for John Pell as a student at Cambridge five years before Wallis, certainly devoted much of his time to mathematics. Wallis did study some astronomy which was then as now a mathematical subject.

After his ordination in 1640 Wallis was employed as a private chaplain, and was also for a short time a Fellow of Queens' College, Cambridge until his marriage in 1645. Always inclined to the puritan tendency in the English church, he was appointed in 1644 as one of the secretaries to the Westminster Assembly of Divines, a body set up to resolve the thorny problems of church

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<sup>2</sup> The most important source of biographical material on Wallis is the autobiography he wrote when he was eighty years old. The full version is in Bodleian Library MS Smith 31, ff. 38-50, and there is also an earlier, shorter draft in British Library Add MS 32499, ff. 375-376<sup>v</sup>, copied again in Bodleian Library MS Eng. misc. e. 475, ff. 256-274. Several eighteenth-century biographies of Wallis were based on this autobiography, and additional material was contributed by David Gregory in 1705, MS Smith 31, ff. 58-59, and William Wallis, great-great-grandson of the mathematician, in 1786, MS Eng. misc. e. 475, ff. 275-349. For a complete list of eighteenth-century biographies and publication details see Scriba 1970, 19-20. See also de Morgan 1838, XXVII, 41-43; Scott 1938 and 1960; Yule 1939; Scriba 1975.

<sup>3</sup> Scriba 1970, 26-27; Fauvel and Gray 1987, 316-317.

<sup>4</sup> Scriba 1970, 40.

<sup>5</sup> Scriba 1970, 27.

government arising from the abolition of the episcopacy in 1643. The outbreak of civil war in England in 1642, however, had already changed Wallis's fortunes in more important ways. In the first year of the war he was shown a ciphered letter in the possession of Sir William Waller, then a colonel in the Parliamentary army. Wallis broke the cipher with ease and such letters continued to come his way not only during the war years but for the rest of his life. In 1653 he was confident about the significance of his work: 'I do not know that there hath been any [letter] deciphered save those that came to my hands; and I believe that if those had escaped my hands, they had likewise escaped that danger.'<sup>6</sup>

After the king's defeat, the University of Oxford was purged of its Royalist supporters, including the two Savilian mathematical professors, and in 1649 Wallis was rewarded for his loyalty to Parliament with the Savilian chair in Geometry.<sup>7</sup> Self-taught from his brother's arithmetic books and William Oughtred's *Clavis*, Wallis knew little enough mathematics when he was appointed, but probably not much less and perhaps rather more than most of his contemporaries, especially after seven years of civil disturbance had brought most normal academic activity to a standstill. Wallis's first great contribution to English mathematics was the new stability and seriousness he brought to the Oxford post; he helped to create an atmosphere in which mathematics was valued and could flourish, and with dedication and commitment rapidly established himself as one of the leading mathematicians of the day. His interest in history was already evident in his inaugural lecture and in one of his first books, his *Mathesis universalis sive arithmeticum opus integrum*, a comprehensive text book on arithmetic, which also included some history of the subject.<sup>8</sup> Within a span of seven or eight years he also worked on the algebraic formulation of conics, on the summation of infinite sequences

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<sup>6</sup> Bodleian Library MS e Musaeo 203, f. xxi. Letters deciphered by Wallis up to 1653 are to be found in Bodleian Library MS e Musaeo 203, copied in MS Eng. misc. e. 475; letters 1651-1701 in British Library Add MS 32499; letters 1689-1703 in Bodleian Library MS Eng. misc. 382. See also Smith 1917, Kahn 1967, 166-169.

<sup>7</sup> Seth Ward was given the chair in Astronomy. See Fauvel, Flood and Wilson 1999, 79-83, 97-99.

<sup>8</sup> Wallis 1657a; Wallis 1657b.

and the quadrature of the circle, on the cycloid and cissoid, and on what later came to be known as number theory.<sup>9</sup> The rapidity and richness of later progress in English mathematics were due in no small part to the foundations laid by Wallis in the 1650s. In the following decade he published a book on mechanics, and wrote many shorter pieces in the form of letters or treatises, often in response to particular problems. In his later years he edited a number of Greek mathematical and musical texts from manuscript. His interest in the history of mathematics, already evident at the beginning of his career continued throughout his life and he was still actively pursuing historical research not long before he died in 1703 at the age of 86.

Wallis held his professorship for over half a century, as well as being *Custos archivorum* (Keeper of the University Archives) from 1657, and towards the end of his life he wrote:<sup>10</sup> 'It hath been my lot to live in a time, wherein have been many and great Changes and Alterations.' He was speaking of political change, but his words applied just as appropriately to mathematics which over Wallis's lifetime had evolved into the notational and conceptual forms familiar to everyone who uses or studies mathematics today. Wallis himself contributed significantly to the development of mathematical thinking in the second half of the seventeenth century, and perhaps nobody was better placed to understand and record the 'Changes and Alterations' that had taken place.

### **The writing of *A treatise of algebra***

Wallis had first stated his intention of writing a text book on algebra as early as 1657 in the final paragraph of his *Mathesis universalis*, where he said that he had hoped to include the 'doctrine of analysis, the perfection of arithmetic' but that he had already written more than he intended. Rather than give too short an account of 'analysis', or algebra, he thought it better to devote a separate volume to the subject, which he was now set to do. Given Wallis's prolific output on other topics it is perhaps not surprising that such a volume did not appear, and there was no further mention of it until some ten years

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<sup>9</sup> For a full list of Wallis's mathematical works see the Bibliography of primary sources.

<sup>10</sup> Scriba 1970, 42.

later. To understand how *A treatise of algebra* came to be written in the early 1670s, and why it remained unpublished until 1685, it is necessary to explore the state of English mathematical publishing during those years, particularly with respect to algebra.

Much of our information on the subject comes from John Collins (1625-1683), government clerk and mathematical enthusiast, and a Fellow of the Royal Society from 1667. Collins not only collected and circulated information about new mathematical texts from abroad, acquiring copies whenever possible, but also did his best to encourage new publication at home. Modest, and untiring in his efforts for others, he is one of the most likeable figures of the seventeenth-century mathematical scene, and has contributed invaluable to our knowledge of the period.

In Holland, the publication of Van Schooten's Latin translation of Descartes' *La géométrie* in 1649 had led to a wave of new research, much of it published in Van Schooten's second, enlarged, edition in 1659-61, and the 1660s saw a number of new text books on Cartesian algebra by Dutch and French writers. In England there was no equivalent upsurge: between 1660 and 1680 the only newly written full length text by an English writer was *The elements of algebra* by John Kersey, published 1673-74. Instead there were attempts to remedy the dearth of good books by reprinting or translating existing texts: Oughtred's *Clavis* was republished in a fourth Latin edition in 1667 but was by then almost forty years old; the Rahn-Pell *Introduction to algebra*, published in 1668, had originally appeared as the *Teutsche Algebra* of Johannes Rahn in 1659, though new material was added by Pell when the book was translated into English by Thomas Brancker during the 1660s.

Adding new material to existing texts served two purposes: it introduced the latest ideas in less time than it took to write a new book, but it also enabled the publication of problems, theorems or short treatises on the back of established texts. Collins' letters are full of suggestions as to what pieces of work might be added to this or that edition, and he was particularly keen to make more and better algebra available to English readers. In 1667, for instance, he suggested that part of Kinckhuysen's *Algebra ofte stel-konst* (1661) should be appended to the Rahn-Pell *Introduction to algebra* then in

the press,<sup>11</sup> but the Rahn-Pell text was published in 1668 without any such additions. Collins then put forward another idea, that Kinckhuysen's text should be printed in full along with extracts from another Dutch algebra, Ferguson's *Labyrinthus algebrae* (1667), and that Wallis might assist in the editing.<sup>12</sup> This too came to nothing, as did the next proposal, that Kinckhuysen's text should be published with notes added by Newton.<sup>13</sup>

The most ambitious of these schemes, again originating with Collins, but also supported by the President of the Royal Society, William Brouncker, was to follow up Kersey's *Elements* with a further volume containing results discovered by Dutch or French mathematicians: Hudde, de Beaune, Bartholin, Dulaurens, Kinckhuysen, Ferguson, Brasser and Verstay.<sup>14</sup> Wallis was again invited to advise and assist, though the work of transcription was to be done for £6 by a friend of Collins, the impoverished Michael Dary. Wallis was willing, though he hoped that 'we find not a stop at the press, which we meet with too often in mathematical books.'<sup>15</sup>

Wallis's remark about the press points to a fundamental reason for the shortage of new books, the reluctance of publishers to undertake mathematical texts which incurred high costs and offered low returns. English booksellers had lost heavily even on the works of such respected authors as Wallis and Barrow and were unwilling to take further risks.<sup>16</sup> There was talk for a while of getting some of Wallis's works reprinted in the Netherlands,<sup>17</sup> but probably the situation was no easier there (and according to Collins the situation in France

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<sup>11</sup> Collins to Pell 9 April 1667; Collins to Brouncker June [1667], Rigaud 1841, I, 126, 136; Scriba 1964, 50.

<sup>12</sup> Collins to Wallis 17 June 1669, Rigaud 1841, II, 515-516; Scriba 1964, 50-51.

<sup>13</sup> Scriba 1964, 51-53; Whiteside 1968.

<sup>14</sup> Collins to Wallis 21 March 1671; Wallis to Collins 14 November 1672; Collins to Wallis 27 March 1673, Rigaud 1841, II, 526, 552, 556. For the Dutch and French authors mentioned by Collins (apart from Verstay whom I have not traced) see Bibliography.

<sup>15</sup> Wallis to Collins 14 November 1672, Rigaud 1841, II, 552.

<sup>16</sup> Collins to Baker 10 February 1677; Collins to Baker 24 April 1677, Rigaud 1841, II, 14, 21.

<sup>17</sup> Wallis to Collins 11 January 1670; Collins to Vernon 4 April 1671, Rigaud 1841, II, 519; I, 161. The idea of publishing Wallis's works in Holland came up again in 1689, see Morland to Wallis 1689, MS Eng. lett. c. 291, ff. 37-38.



was even worse<sup>18</sup>). Wallis, Barrow and others had adopted the habit of depositing their papers with the Royal Society until an opportunity for publishing arose, and by April 1677 Collins had held some of Wallis's papers for several years.<sup>19</sup>

We do not know exactly when Wallis began to write *A treatise of algebra*. In 1668 he had written that 'an Introduction to Algebra I have not yet ready',<sup>20</sup> and it seems unlikely that he would have agreed to assist Collins and Dary in 1672 if he was already engaged on a major work on algebra himself, so we may suppose that the draft was not begun earnest until 1673 and that Wallis continued to work on it until he delivered it to Collins in 1677. The dating 1673 to 1677 is confirmed by a number of mathematical letters which Wallis wrote in response to queries from Collins in 1673 and 1676, and then included in *A treatise of algebra*.<sup>21</sup> Newton's *Epistola prior* and *Epistola posterior*, extracts of which were published for the first time in *A treatise of algebra*, were written in June and October 1676, and it is possible that the long section on Wallis's *Arithmetica infinitorum* in which the Newton extracts are embedded was not written until the winter of 1676-77.

Collins died in 1683 but by then the Royal Society had promised to underwrite the publication of *A treatise of algebra* and a deal had been negotiated with Richard Davis, an Oxford bookseller, who agreed to handle it if sufficient sales were guaranteed. Down payment on 100 copies seems to have been the necessary level of support,<sup>22</sup> and the Royal Society undertook to buy 60 copies at 1½d per sheet and invited further subscriptions at the same rate. A *Proposal* to publish *A treatise of algebra* was circulated in 1683;<sup>23</sup> it invited subscribers to send a deposit of five shillings before December 1683 and promised to print at a rate of two sheets a week from 1 August 1683. (The

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<sup>18</sup> Collins to Baker 10 February 1677, Rigaud 1841, II, 15.

<sup>19</sup> Collins to Baker 24 April 1677, Rigaud 1841, II, 21.

<sup>20</sup> Wallis to Collins 3 November 1668, Rigaud 1841, II, 507.

<sup>21</sup> Wallis to Collins 29 March, 8 April, 12 April, 6 May, 27 September 1673, 11 September 1676, Rigaud 1841, II, 557-586, 591-600.

<sup>22</sup> Collins to Baker 24 April 1677, Rigaud 18941, II, 21.

<sup>23</sup> The *Proposal* was reprinted at the beginning of *A treatise of algebra* following the (unpaginated) preface and contents.

book eventually required four quires, or 96 sheets, of paper, and so cost twelve shillings to subscribers but sixteen shillings or more to later buyers.) The book was printed by John Playford in London, who possessed the necessary range of type,<sup>24</sup> and it was eventually completed not in 1684, as hoped, but in 1685, some twelve years after Wallis first began to write it.<sup>25</sup>

The delay between writing and printing gave Wallis plenty of opportunity to add to his text and he continued to do so up to the last possible moment, the final ‘Additions and Emendations’ being written in 1684 when much of the book was already printed. The opportunity to publish was too valuable to waste and prompted Wallis to include as much as he could of the work deposited with Collins, some of it in appendices, some absorbed into the main text. This makes *A treatise of algebra* read at times like an anthology of results and ideas with little obvious relation to one another. Running through the text, nevertheless, is a clear historical thread. At times it seems in danger of vanishing but Wallis always managed to bring it back into focus: the history which was incidental to his *Mathesis universalis* thirty years earlier had now become his main theme.

### **The contents of *A treatise of algebra*, and the structure of the thesis**

The main text of *A treatise of algebra*, without appendices but including the *Additions and emendations*, runs to just under 400 pages divided into a hundred chapters as follows:

- Chapters 1-2      Hints of algebra in Classical and Islamic writers; how such ideas spread to Europe, especially England.
- Chapters 3-12    The development of the numeral system from Archimedes to the seventeenth century, with particular emphasis (Chapters 3 and 4) on the arrival of the Hindu-Arabic numeral system in northern Europe.

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<sup>24</sup> *Treatise of algebra*, Preface.

<sup>25</sup> Such long production times were not at all unusual. Kersey’s *Elements* took some twelve years to write and a further six or seven to publish. Bulliald’s *Opus novum*, 1682, Clark’s *Oughtredus explicatus*, 1682, and Baker’s *Geometrical key*, 1684, were all begun twenty to thirty years before they were eventually published.

- Chapters 13-14 The development of algebra from Leonardo of Pisa to François Viète.
- Chapters 15-29 The algebra of William Oughtred, and applications.
- Chapters 30-56 The algebra of Thomas Harriot, and applications.
- Chapters 57-72 The algebra of John Pell and miscellaneous related topics.
- Chapters 73-97 Wallis's *Arithmetica infinitorum* and work derived from it by Isaac Newton and others.
- Chapters 98-99 Methods in number theory developed by William Brouncker and Wallis.
- Chapter 100 Conclusion.

Everything from Chapter 15 onwards is about the work of seventeenth-century English mathematicians, with most of whom Wallis was personally acquainted, and though never explicitly stated it seems that Wallis's purpose from the start was to extol the achievements of his own countrymen and his own time. The first fourteen chapters trace the Classical, Islamic and Renaissance precursors to the later English flowering: in their content and analysis they are uneven, containing both the best and the worst of Wallis's historical writing, and have generally been overlooked by later commentators but include much interesting material.

The early chapters set the scene not just historically but mathematically: Wallis was throughout his career an arithmetician rather than a geometer and for him algebra was an extension of arithmetic. It was therefore natural for him to begin a history of algebra with a brief history of the numeral system without which, he argued, algebra could not conveniently be managed.<sup>26</sup> Only once did he come close to defining algebra in its own right, as being chiefly concerned with solving equations,<sup>27</sup> but towards the end his work ranges far beyond such a limited definition. It was Wallis's stated aim 'to consider pure Algebra from its own Principles; abstracted from Geometry and other Accommodations to particular Subjects';<sup>28</sup> on these grounds he largely avoided

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<sup>26</sup> *Treatise of algebra*, 15.

<sup>27</sup> *Treatise of algebra*, 128.

<sup>28</sup> *Treatise of algebra*, 272.

any discussion of algebra in relation to geometry,<sup>29</sup> though he could not resist putting in a few examples to demonstrate the applicability of the methods of Oughtred, Harriot or Pell.

Neglect of ‘Accommodations’ was one of the few restrictions Wallis introduced; in every other respect he followed wherever his subject led him, so that his text is a mixture of historical survey, mathematical demonstration, textual criticism, commentary and polemic. This has led to its readers interpreting it according to their own circumstances or prejudices: as a text book on algebra, as a repository of interesting mathematics, or as history, good or bad. The present thesis revisits Wallis’s text and, as previous readers have done, engages with it in a variety of ways. It is now possible to elucidate or correct parts of Wallis’s account in the light of later research and a longer historical perspective, and this I have attempted to do where appropriate. At one level, therefore, this thesis contributes to the modern study of the evolution of algebra from the twelfth to the seventeenth century. My account, like Wallis’s, is derived as far as possible from a wide range of primary sources and in this, again like Wallis, I have been privileged in having access to the rich resources of Oxford’s Bodleian Library.

The thesis is also, however, a study of historiography. Perceptions of historical change and methods of discovering historical truth changed dramatically during the seventeenth century, and *A treatise of algebra* gives us a direct insight into the way the history of mathematics came to be seen from Oxford towards the end of that century. Wallis can with some justification be claimed as the first modern historian of mathematics, and here for the first time his sources and his methods are analysed in some detail. That Wallis himself had created part of the history he was describing, and that he wrote into it his personal preferences and prejudices, far from invalidating his account lures us all the more fully into his world.

The thesis therefore combines different levels of analysis, with varying degrees of emphasis according to the subject matter. In general I have adhered

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<sup>29</sup> Newton at about the same time similarly eschewed the use of algebra in geometry, Newton 1967-81, V, 429.

as closely as possible to Wallis's own structure and have divided my chapters as follows:

Thesis	Contents	<i>A treatise of algebra</i>
Chapter 1	Introduction to <i>A treatise of algebra</i> .	
Chapter 2	Wallis's account of mathematical learning in medieval England.	Chapters 2-4
Chapter 3	The development of algebra from Leonardo of Pisa to Viète.	Chapters 1, 13-14
Chapter 4	The algebra of William Oughtred, its influence on Wallis, and Wallis's role in promoting the <i>Clavis</i> .	Chapters 15-29
Chapter 5	The algebra of Thomas Harriot and the attempts of Wallis and Pell to further Harriot's reputation	Chapters 30-54
Chapter 6	The mathematics of John Pell.	Chapters 10-11, 55-72
Chapter 7	Wallis's <i>Arithmetica infinitorum</i> and work arising from it, including the development of infinite series and the binomial theorem by Newton.	Chapters 73-97
Chapter 8	The mathematical work of William Brouncker: his infinite fraction for $4/\pi$ and his response to Fermat's challenges.	Chapters 83, 98-99
Chapter 9	Conclusion.	

Omitted are Wallis's Chapters 5-9 and 12 on Archimedes' representation of large numbers,<sup>30</sup> on sexagesimal and decimal fractions, and logarithms, these

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<sup>30</sup> Wallis 1676.

being topics which, as Wallis recognised, belong more properly to arithmetic than to algebra. Some of the chapters of the thesis draw together information from disparate sources to create a fresh picture; others explore newly discovered or little known material. New material or analysis is offered in the following areas:

- (Chapter 2) Wallis's history of mathematical learning in medieval England is explored in detail for the first time, tracing the sources he used, almost all of which can still be found in Oxford libraries. An exciting material find from this period is the mantelpiece which, Wallis claimed, bears one of the earliest examples of Hindu-Arabic numerals in England; it is hoped that modern dating methods will confirm or discount Wallis's conjecture.
- (Chapter 3) From the Bodleian Library's superb collection of sixteenth-century algebra texts, this chapter presents a new analysis of the different ways algebra was perceived and practised in that century, and how the various strands eventually combined to form algebra as we know it today. Appended is a bibliography of cossist algebras up to 1600.
- (Chapter 4) The story of Oughtred's *Clavis*, which spanned seventy years in seven editions, is reconstructed from contemporary sources and set into the context of seventeenth-century mathematics and politics. The chapter also includes discussion of all algebras published in England up to 1660 and a bibliography of English algebras up to 1702.
- (Chapter 5) This chapter explores the fate of Harriot's algebra after his death in 1621, and draws attention to little known copies of his work which have survived. It also brings to light for the first time the crucial role of Pell in Wallis's controversial account of Harriot. I offer a reappraisal of Harriot's algebra from his own manuscripts and those of his friend and colleague Nathaniel Torporley.
- (Chapter 6) Pell has long been one of the most enigmatic of the seventeenth-century mathematicians, but fresh research among his papers has revealed that some of his mathematics made its way into *A treatise of algebra*, and that his influence on Wallis was considerable. Another new find is a series of intercepted letters from 1650 written in code by Pell and

deciphered by Wallis, an exchange of both political and mathematical significance.

- (Chapter 7) Previous accounts of Wallis's *Arithmetica infinitorum* have focused on its results rather than its methods, and few (apart from Wallis himself) have appreciated the full extent of the book's influence on seventeenth-century English mathematics, or its role in the eventual transformation of mathematics into the language of algebra. This chapter looks anew at the contribution and significance of the *Arithmetica infinitorum*.
- (Chapter 8) Close examination of Brouncker's few published results, in *A treatise of algebra* and other works by Wallis, reveals him as a gifted and intuitive mathematician. His derivation of a series of continued fractions for multiples of  $\pi$  has remained a mystery to later mathematicians and this chapter offers a possible reconstruction of his method. The chapter also analyses Brouncker's response to Fermat's number challenges and his importance for the later development of number theory.





## Chapter 2

### Of our own nation: John Wallis's account of mathematical learning in medieval England

#### Summary

In *A treatise of algebra* Wallis wrote the first survey of the state of mathematical learning in medieval England, and discussed with particular care the arrival and significance of the Hindu-Arabic numeral system. This chapter offers a detailed commentary on Wallis's account in relation to the sources he used and the seventeenth-century Oxford context in which he wrote. The chapter also supplements Wallis's treatment where possible with some of the findings of modern scholarship. It therefore provides on the one hand an overview of the spread of mathematical learning into medieval England, and on the other an insight into late seventeenth-century historiography. Wallis pioneered several new historiographical methods and can perhaps be claimed as the first modern historian of mathematics.

Commentators on *A treatise of algebra* have almost completely ignored Wallis's account of the medieval period,<sup>31</sup> yet in it Wallis displayed greater objectivity and a truer sense of the complexities of historical development than in almost anything else he wrote. In his opening chapters, Wallis showed himself at his finest as a historian, and introduced new methods and standards of research that entitle him to be considered the first modern historian of mathematics.

A unique combination of circumstances in seventeenth-century Oxford made Wallis's research possible. Histories of mathematics are perhaps written

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<sup>31</sup> Wallis's chief biographer, J. F. Scott, disposed of these early chapters in one sentence: '[Wallis's] account of the history of mathematics in antiquity is very comprehensive and gives evidence of a close study of the Classical literature of the sciences'. Scott made no mention of Wallis's researches on the medieval period: Scott 1936, 335, reprinted as Scott 1938a, 133. For commentary on selected paragraphs from Wallis's Chapters 1,2 and 6 see Molland 1994, 215-218.

only when mathematicians perceive marked changes in the nature and scope of their subject,<sup>32</sup> and by the second half of the seventeenth century it was plain that mathematics was steadily liberating itself from the constraints of its Classical past and taking on a life and momentum of its own. Wallis had seen this revolution at first hand during his long tenure of the Savilian professorship, and indeed had done much to bring it about. He was also well placed in a second and more material way, through his access to the unprecedented accumulation of books and manuscripts in Oxford's Bodleian Library. From the opening of the library in 1602 there had been energetic and wide-ranging efforts to collect and preserve texts from England and abroad (see Appendix I: Seventeenth-century Bodleian collections),<sup>33</sup> and the concentration of this wealth of material in a single place both reflected and encouraged new attitudes to historical study. The range of Wallis's reading will become evident in the course of this chapter: his longstanding interests in grammar, etymology, cryptanalysis, music, astronomy, calendar reform and general history all informed his account of the medieval period. He knew his Classical sources thoroughly but also recognised, thanks to the new proliferation of oriental studies in Oxford,<sup>34</sup> the debt of European mathematics to Indian and Islamic sources, and the main theme of Chapters 2-4 of *A treatise of algebra* was the transmission of learning from Islamic Spain to northern Europe. Some of the material had already appeared thirty years before in his *Mathesis universalis*<sup>35</sup> but there was also much in *A treatise of algebra* that was new.

This present chapter follows Chapters 2-4 in some detail and with a double purpose: first, to discover what was known and understood during the seventeenth century about mathematics in the medieval period; second, to look at Wallis's methods of research, and his establishment and use of new historiographical techniques. Each of Wallis's paragraphs, numbered for ease

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<sup>32</sup> The first history of mathematics was written by Eudemus (late fourth century BC) who, like Wallis in the seventeenth century, was aware of the many new discoveries made by his predecessors. See Fauvel and Gray 1987, 46-47.

<sup>33</sup> For a fuller account of the Bodleian Library at this period see Philip 1983.

<sup>34</sup> Russell 1994; Toomer 1996; Feingold 1997.

of reference, will be quoted in full followed by an accompanying commentary.<sup>36</sup> The story is taken up part way through Chapter 2, at paragraph 10, where having briefly considered what (slender) evidence of algebra could be prised from the writings of Euclid, Archimedes, Pappus and Diophantus,<sup>37</sup> Wallis turned to Arabic mathematics.

§ 2.10 After *Diophantus* (if not before, also) this learning was pursued by *Arabic* authors (but little known in *Europe* for a long time). From them it had the name of *Algebra*; not (as some would have it) from *Geber*, whom they conjecture (without any good ground that I know of) to have been its first inventor; but (as was said before) from its *Arabic* name, *Al-gjābr W'al-mokābala*.

The term ‘Arabic authors’ here and throughout should be taken, as Wallis intended, to mean writers from anywhere in the Islamic world who used Arabic, the common language of Islamic culture.

Immediately apparent in this paragraph is Wallis’s interest in word derivation. It was a longstanding preoccupation: his reputation as a mathematician has overshadowed the fact that in 1653 he published a highly regarded treatise on English grammar,<sup>38</sup> a substantial chapter of which was concerned with etymology. He was correct in tracing the word *algebra* to the *Al-jabr wa'l muqābala*, the seminal text of Muḥammad ibn Mūsā al-Khwārizmī (c.770-850).<sup>39</sup> On the opening page of *A treatise of algebra* Wallis had already traced the meanings of the Arab words *gjābara* (to restore or set a broken bone) and *kābala* (to set one thing against another).<sup>40</sup> A literal translation of the title would be ‘Restoration and balancing’, perhaps

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<sup>35</sup> Wallis 1657b, Chapters 6-9.

<sup>36</sup> Wallis made a number of handwritten corrections and annotations to his text in his own copy of *A treatise of algebra* (Savile A.3). Most are corrections of typographical errors and are incorporated in the transcripts given here without comment. Lengthier annotations are shown in curly brackets { }.

<sup>37</sup> The existence of a Greek ‘geometrical algebra’ has been discussed at some length during the twentieth century: see, for instance, Unguru 1975, 1979; Van der Waerden 1976; Freudenthal 1977; Weil 1978; Mueller 1981, 43-44, 50-52; Berggren 1984, 394-410.

<sup>38</sup> Wallis 1653.

<sup>39</sup> See Karpinski 1915; Chapters 1-6 of Karpinski’s English translation are reprinted in Grant E. 1974, 106-111.

<sup>40</sup> *Treatise of algebra*, 2.

originally referring to the well known method of solving a quadratic equation by completing (*restoring*) a geometric square and comparing (*balancing*) the result with a known quantity, though later the terms were used of operations on equations.<sup>41</sup> The mistaken association of algebra with Geber (the twelfth-century astronomer Jābir ibn Aflaḥ) was made by Girolamo Cardano (1501-1576) who, in his list of twelve great scientists noted Mahometius Mosis filius (al-Khwārizmī) as the inventor of algebra but supposed that as a result of his invention he took the name Geber.<sup>42</sup> Wallis had earlier considered the possible identity of al-Khwārizmī and Geber, but seems to have confused Jābir ibn Aflaḥ with the early ninth-century alchemist Jābir ibn Ḥayyān, and had been unable to draw any firm conclusion;<sup>43</sup> here he rejected such a hypothesis.

§ 2.11 Divers writers ('tis said) there are of *Algebra* in that Language, and from them (I suppose) the Denominations of *Diophantus* (if from him they learned it) came to be changed; and (beside the Denominations of Root, square, and cube,) that of *Sursolids* (first, second, third, &c.) introduced. But I rather think the *Arabs*, either of themselves, or from some others, had it long before *Diophantus*, and think this reckoning of *Powers* (by *Sursolids*, &c.) different from *Diophantus*{, to be a good Argument for it.}

Wallis had already described Diophantus' method of naming powers as *Movάς* (unit), *Αριθμός* (number), *Δύναμις* (power, or square), *Κύβος* (cube), denoted by  $\mu$ ,  $\varsigma$ ,  $\delta$ ,  $\kappa$ ,  $\delta\delta$ ,  $\delta\kappa$ ,  $\kappa\kappa$  and so on.<sup>44</sup> This was an *additive* system in which

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<sup>41</sup> Saliba 1973.

<sup>42</sup> Cardano 1553, 1011f. The writers listed by Cardano were: Archimedes, Aristotle, Euclid, Ioannes Scotus, Ioannes Suisset, Apollonius of Perga, Archytas Tarantinus, Mahometus Mosis filius, Alchindus, Heber [Geber] Hispanus, Galen, Vitruvius. Wallis read and annotated Cardano's list in the Savile Library copy, now Savile S.11. The identity of Geber is not obvious from Cardano's list but in his *Encomium geometriae*, Cardano 1663, IV, 443, Cardano referred to Geber as a writer on triangles and circles, a clear reference to Jābir ibn Aflaḥ. See also Cifoletti 1996, 128; Høyrup 1996, 113. For other ascriptions of algebra to Geber see Cifoletti 1996, 128-135.

<sup>43</sup> *Treatise of algebra*, 4. Wallis knew Jābir ibn Aflaḥ's commentary on Ptolemy's *Almagest*, published at Nuremberg in 1534, in the copy now known as Savile X.3, but despite this thought that Jābir ibn Aflaḥ lived in the ninth century. The alchemical writings of Jābir ibn Ḥayyān (late eighth to early ninth century) were published in London in 1686. Wallis discovered additional information on al-Khwārizmī in Abū 'l-Faraj 1663, 161; Eutychius 1656, II, 447, both translated by the Oxford Arabist Edward Pococke.

<sup>44</sup> *Treatise of algebra*, 4; Heath 1931, 476-478.

higher powers are expressed as sums of preceding ones. Wallis supposed that the Arab writers introduced the alternative *multiplicative* system<sup>45</sup> in which powers were called ‘root’ (*R*), ‘zensus’ (from *census*, literally wealth or excess, for a square, denoted *Z*), ‘cubus’ (*C*), ‘zenso-zensus’ (*ZZ*), ‘first sursolid’ (e.g.  $\int Z$ ), ‘censo-cubus’ (*ZC*), second sursolid’ (e.g.  $b \int Z$ ), and so on,<sup>46</sup> where every prime power has to be given a new name and symbol. This was the system generally used by the sixteenth-century cossist writers, and only with the rediscovery of Diophantus did the additive system come back into use alongside it (leading to potential confusion as to whether  $A_{qc}$  meant  $A^5$  or  $A^6$ ). Wallis later in *A treatise of algebra* claimed that the multiplicative system was used by ‘all our European Algebrists before Vieta, having learned it from the Moors’,<sup>47</sup> but in this he was mistaken: both systems were used in fifteenth-century Italy in the earliest attempts to deal with powers higher than three.<sup>48</sup>

§ 2.12 With the *Arabs* all sorts of Mathematical Learning flourished, and was improved, for a long time together, while in *Europe* it was very much neglected. Amongst whom were *Maimon*, *Almeon*, *Alchindus*, *Albumasar*, *Alfraganus*, *Alfarabius*, *Geber*, *Mahometes Bagdadinus*, *Mahometes ben Musās*, *Thebit*, *Haly*, *Alchabitius*, *Alhazen*, and divers others. To whom I may add also some *Persians* and *Tartars*, as *Al-suphi*, *Nasir-eddin*, *Shah-colgius*, *Uleg-beig*, &c. whose Astronomical Tables are yet in being.

At several points in his account Wallis presented, as here, a list of names with little additional information: in this case there is not even a date. In every case such lists were drawn from Wallis’s main source for this period, the *De scientiis mathematicis* of John Gerard Vossius (1577-1649), the third book in his trilogy on contemporary arts and sciences.<sup>49</sup> Vossius was a renowned

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<sup>45</sup> Wallis 1657b, Chapter 11.

<sup>46</sup> The exact symbolism varied from writer to writer but this scheme, from *Recorde* 1557, is typical.

<sup>47</sup> *Treatise of algebra*, 91.

<sup>48</sup> Reich 1994, 193.

<sup>49</sup> Vossius 1650. The title at the beginning of the third book, on mathematics, is *De universae matheseos natura ac constitutione liber; cui subjungitur chronologia mathematicorum*, different from the *De scientiis mathematicis, cui operi subjungitur, chronologia mathematicorum* on the main title page. For convenience I will use, as Wallis did, the abbreviated title *De scientiis*. Wallis owned a copy of the 1660 edition, now Bodleian

Dutch scholar who had spent some years in England from 1629 to 1633 (and had been a canon of Canterbury) but had returned to take up the chair of history in the new university of Amsterdam. There he knew the English mathematician John Pell, who taught in Amsterdam from 1643 to 1646, and it may have been Pell who introduced the work of Vossius to Wallis. In *De Scientiis* descriptions and histories of different branches of mathematics (arithmetic, geometry, logistics, music, optics, and so on) are followed by an extensive chronological list of mathematicians, for each of whom Vossius gave, as far as possible, a date and details of extant works. Vossius was not, however, a mathematician and did not discuss mathematical content. As he said himself:<sup>50</sup> *Neque enim ipsam tradimus scientiam; sed de ea scribimus* ('Nor do I teach the science itself, but only write about it'). His information was drawn from other authorities (whom he cited frequently); his own contribution was to collate and order the facts at his disposal.

Wallis's copy of *De scientiis* is preserved in the Bodleian Library and his frequent and detailed annotations show how thoroughly he read it. The Arabic writers listed in § 2.12 are all to be found in *De scientiis* though not in the order given by Wallis. The first to appear is *Mahometes ben Musas* (al-Khwārizmī, c.770-850) whom Vossius mentioned briefly at the end of his account of Greek and Latin writers on arithmetic.<sup>51</sup> Vossius seems to have used Cardano as his source here: both referred to al-Khwārizmī by his given names as Mahomet son of Moses (*Mahomet Mosis filius*), and Vossius stated that Cardano listed Mahomet ninth (actually eighth) in his list of twelve great scientists.<sup>52</sup> A few pages later *Mahomet Bagdadinus* (al-Baghdādī, fl. c.1230) and *Alchindus* (al-Kindī, c.801-c.866) were described by Vossius as writers on

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Library Savile G.21, and my references are to this edition. For the life and work of Vossius see Rademaker 1981.

<sup>50</sup> Vossius 1650, 37.

<sup>51</sup> Vossius 1650, 41.

<sup>52</sup> See note 42. Together with Mahometes ben Musas, Vossius also mentioned one Abraham Cai, a Jew. In the index to *De Scientiis* Abraham instead of Mahomet is wrongly described as the inventor of algebra and ninth in Cardano's list. Wallis made a handwritten correction in his copy.

geometry,<sup>53</sup> and then *Alchabitius 1410* (al-Quabīsī, fl. c.950) and *Alhazen 1100* (al-Ḥasan or ibn al-Haytham, 965-c.1040) as writers on optics.<sup>54</sup>

The remaining writers: *Maimon 827* and *Almeon 838* (both references to caliph al-Ma'mūn, 809-883, founder of the House of Wisdom in Baghdad),<sup>55</sup> *Albumasar 884* (Abū-Ma'shar, c.810-886), *Alfraganus 879* (al-Farghānī, d.861), *Alfarabius 940* (al-Fārābī, c.870-950), *Geber* (Jābir ibn Aflaḥ, fl. 1145), *Thebit 1300* (Thābit ibn Qurra, 836-901) and *Haly 1202* (Abū-'l-Ḥasan, fl.1020-1040) are all listed as astronomers.<sup>56</sup> Also in Vossius but curiously missing from Wallis's list are *Albategnius 888* (al-Battānī, 850-929), *Arzachel 1080* (al-Zarquālī or Azarquiel, d.1100) and *Abenezra 1145* (Rabbi Abraham ben Meir ibn Ezra, 1092-1167). As Wallis mentioned all three later in connection with their astronomical tables, their exclusion from this preliminary list was perhaps deliberate. There are no other omissions: every Arab writer recorded by Vossius was also noted by Wallis. In Wallis's copy of *De scientiis* the pages on Arab astronomy are particularly heavily annotated<sup>57</sup> and it is clear that he read them carefully. But he also extracted Arab writers from other sections and ordered the entire list more or less chronologically for his own text.

Wallis's brief list of 'Persians and Tartars' came from a different source. In 1648 John Greaves, a scholar of both Persian and Arabic, and Savilian Professor of Astronomy (1643-1649) had published the geographical tables of the Persian Naṣīr al-Dīn al-Ṭūsī (1201-1274) and of the 'Tartar' Ulugh Beg

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<sup>53</sup> Vossius 1650, 61. The *De superficierum divisionibus* of al-Baghdādī was published by Commandino in 1570 from a manuscript supplied by John Dee, who conjectured that it was a lost book of Euclid, though there is in fact only an indirect connection with Euclid's work.

<sup>54</sup> Vossius 1650, 109.

<sup>55</sup> Vossius mentioned a second, later, *Maimon* who can be identified as the philosopher Rabbi Moshe ben Maimonides (1135-1204). Wallis omitted him, perhaps incorrectly assuming duplication.

<sup>56</sup> Vossius 1650, 173-181.

<sup>57</sup> Page 177, in particular, from al-Farghānī to ibn Ezra, is heavily annotated at every paragraph.

(1394-1449), king of Samarkand and founder of its observatory.<sup>58</sup> Two years later Greaves translated and published astronomical and chronological tables of Ulugh Beg, and the astronomy of the Persian al-Kāshī (Shah-colgius) (d. 1429) who assisted Ulugh Beg in Samarkand and made improvements to the astronomical tables of Nāṣir al-Dīn al-Ṭūsī.<sup>59</sup> The catalogue of fixed stars compiled by Ulugh Beg also drew on the earlier observations of Al-Sūfī (903-986).

Wallis made no attempt to describe for any of these writers their individual work or even their field of study though he could easily have done so. To the modern reader his list of names, devoid of historical context, raises far more questions than it answers, but Wallis was following a long established paradigm of historical writing, which concentrated on authors rather than ideas, and on stability rather than change. The underlying assumption, though one that Wallis could no longer completely share, was that mathematical knowledge derived from divine revelation or ancient authority, so that the history of mathematics was essentially the handing on (*traditio*) of such knowledge from one generation or culture to the next.

There are several histories of this kind by medieval and Renaissance writers. Assertions that mathematics was handed from the Babylonians or the Hebrews to the Egyptians and thence to the Greeks can be seen in the earliest post-Classical histories, those of Isidor of Seville (570-636)<sup>60</sup> and Bede (672-735).<sup>61</sup> Later medieval accounts became more sophisticated but presented much the same story. By the thirteenth-century Bacon saw the history of science as a process of decline in which ancient knowledge was occasionally recovered only to be lost again.<sup>62</sup> Two centuries later, in 1464, Regiomontanus wrote a history of mathematics in which the main theme was not change, but the continuity and stability of mathematics as handed from one mathematician

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<sup>58</sup> Greaves 1648. The Tartars came originally from the east Asian steppes; the description is probably used indiscriminately here for the various Mongol tribes which overran central Asia in the early thirteenth century.

<sup>59</sup> Greaves 1650a; Greaves 1650b.

<sup>60</sup> Isidor, Migne LXXXII, cols 153-184; 155-169.

<sup>61</sup> Bede (ascribed), Migne XC, cols 647-653; 650.

<sup>62</sup> Molland 1995, 214, 221-223.



to another.<sup>63</sup> Cardano's 1553 list of twelve great writers has already been mentioned: there six Greeks, two Britons, a Roman and three Arabs are ordered by eminence rather than chronology, and without any suggestion of historical development or context. Bernardino Baldi (1553-1617), in his *Vite de matematici* written almost at the end of the sixteenth century<sup>64</sup> still conveyed mathematics as a continuous tradition running from the Babylonians (Chaldeans) and Egyptians through the Greeks, Romans and Arabs to his own time, in which the greatest achievement was the restoration of Archimedes, and his *Cronica de matematici* listed an unbroken line of mathematicians from 600 BC to 1596.<sup>65</sup> A similar list, which was available to Wallis but which he appears not to have used, was compiled by Henry Savile in 1570 and is preserved in the Savile Library. Savile's list began with the sons of Seth and continued through the Druids and Zoroastrians to Abraham, Joseph, Homer and Pythagoras before reaching the firmer historical ground of Classical Greece.<sup>66</sup> Savile, like his predecessors, was chiefly concerned with demonstrating the deep roots of mathematics in its Classical past.

Only very gradually did there begin to emerge ideas of mathematical progress, a sense that modern mathematicians could add to or even improve upon the existing body of knowledge.<sup>67</sup> Writing in the 1640s, Vossius still presented the various branches of mathematics as existing largely independently of age or culture, so that detailed tracing of ideas was less important than identifying the carriers of the tradition, who did not themselves need to be innovators. Wallis's lists of names, here and elsewhere, were quite compatible with this established style of historical writing; it makes his new methodology later all the more remarkable.

§ 2.13 From those *Arabians* we have the names of *Almagest*, *Azimuth*, *Almicanter*, *Zenith*, *Nadir*, *Almanack*, *Algorism*, *Algebra*, &c. and divers other *Arabic* words (now disused) we find retained in *Regiomontanus*,

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<sup>63</sup> Regiomontanus 1464; Rose 1975, 95-98.

<sup>64</sup> Baldi 1998; Rose 1975, 253-269; Moyer 1999.

<sup>65</sup> Baldi 1707.

<sup>66</sup> MS Savile 29; Goulding 1999, 123-125. Savile presented sound historical arguments about the identity of Euclid, see Goulding 1999, 96-103.

<sup>67</sup> Zissel 1945; Lilley 1958, 3-37; Crombie 1975; Molland 1978; Molland 1983, 141-148.

*Purbachius* and others before them, who either translated *Arabic* Authors, or at least derived their Learning from them. As I find in divers of those Manuscript Authors, which I have seen, concerning the *Astrolabe* (whose Parts they describe by *Arabic* names), and other Mathematical Learning.

Here again Wallis's interest in etymology is evident and he was correct in tracing all these words to Islamic scientific, and especially astronomical, writing. Georg Peurbach (1423-1461), humanist and astronomer of the University of Vienna, was the teacher of Johannes Muller, Regiomontanus (1436-1476). They were regarded by their contemporaries as being responsible for the renaissance of astronomy in Europe, and Regiomontanus completed a translation and critique of Ptolemy's *Almagest* begun by Peurbach.<sup>68</sup> The Savile Library held a copy of the 1550 edition which would have been known to Wallis.

§ 2.14 They translated *Euclid*, *Ptolemy*, *Aristotle*, and divers others of the *Greek* Authors into *Arabic*; and out of the *Arabic* we had our first Translations of *Euclid*, *Ptolemy*, and other *Greek* Authors, into *Latin*, before those out of the *Greek*. A thing of it self notorious, and so also attested by *Vossius*, (after *Sir Henry Savil*;) *Euclidem Latini Translatum habuerunt prius ex Arabico quam ex Graeco fonte. Quemadmodum & ante CC, & infra, annos, non alia Aristotelis, Galeni, Ptolemaei, aliorumque multorum, interpretatio in manibus erat, quam ex Arabica versione Latine, vel Semibarbaro, potius, expressa.* And by *Sir Henry Savil*, in his second Lecture on *Euclid*, almost in the same words. And from them we received not only our *Algebra*, but other parts of Mathematical Learning; brought by the *Moors* into *Spain*, and from thence propagated to other parts of *Europe*; about the year of our Lord 1100, or somewhat sooner.

The passage from Vossius translates as: 'They had a Latin translation of Euclid from Arabic before any from a Greek source. Just as, for up to two hundred years before that, they had no translation of Aristotle, Galen, Ptolemy or many others, other than Latin, or rather semi barbarous, versions from Arabic'. Vossius did not acknowledge Savile as the source of his information but Wallis clearly knew Savile's 1619 lectures on Euclid well, and recognised the relevant passage:<sup>69</sup> *Et quidem nos occidentales Europaei Arabibus primus omnium debemus Aristotelem, Euclidem, Galenum, Ptolemaeum, caeteros Graecorum Principes, cum ante centum annos aliae versiones nullae,*

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<sup>68</sup> Regiomontanus 1550; Rose 1975, 90-94.

<sup>69</sup> Savile 1621, 35. Savile presented the Bodleian Library with an inscribed copy of his *Praelectiones*, now 4<sup>o</sup> S.39 Art; Savile's original manuscript is also preserved, as MS Savile 37.

*praeterquam ex Arabico, fuerint in manibus nostrorum hominum, quod Graecae linguae cognito nondum in Italiam et Occidentem immigrasset.* Wallis assumed that this information was already well-known (notorious).

Wallis was less than careful here and elsewhere in distinguishing between different periods and geographical locations of Islamic culture, and probably used 'Moors' in the general sense of 'Muslims'. Mathematical learning was brought not only by the true Moors, the north African invaders and settlers of Spain, but also by later travellers and scholars from elsewhere in the Islamic world, from the old Hellenistic regions of the eastern Mediterranean and the new centre of learning at Baghdad.<sup>70</sup>

The first Latin translations of Euclid from Arabic were made by Adelard of Bath (c.1130), Hermann of Carinthia (c.1143) and Gerard of Cremona, who also did the first translation from Arabic of Ptolemy's *Almagest* (c.1175). In fact a translation of the *Almagest* directly from Greek was done in Sicily in 1165 by an anonymous student from Salerno who has also been credited with a translation of Euclid's *Elements* from Greek, but neither translation was well known until the twentieth century and would not have been known to Savile, Vossius or Wallis.<sup>71</sup>

§ 2.15 Upon this account, I find that divers of our own Nation, about the twelfth and thirteenth Century, (not satisfied with the Philosophy of the Schoolmen,) were inquisitive into the *Arabic* Language, and the Mathematical Learning therein contained.

Wallis was here, perhaps deliberately, pointing to the medieval forebears of the seventeenth-century Oxford interest in Arabic language and science. He was also introducing two new themes he was about to explore in detail. A more precise determination of the date of transmission of Arabic learning through Spain to the rest of Europe was to be the subject of his next two chapters. The role of Englishmen in acquiring and propagating the new ideas, and the subsequent revitalisation of English mathematics fills the remainder of Wallis's Chapter 2.

§ 2.16 As *Adelardus*, (a monk of *Bath*) whom *Vossius* placeth about the year 1130 who for that purpose travelled into *Spain*, *Egypt*, and *Arabia*; and

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<sup>70</sup> Fletcher 1992.

<sup>71</sup> Rose 1975, 76-79.

(as Vossius tells us) translated *Euclid* (and some other *Arabic* authors) out of *Arabic* into *Latin*, Anno hoc MCXXX. *Athelardus sive Adelardus, Anglus, Monachus Bathoniensis, Euclidis Geometriam ex Arabico vertit Latine. Nec, Arabice scivisse, mirandum: Quando non modo Galliam, Germaniam, Italiam, adiit; sed etiam Hispaniam, Ægyptum, Arabiam ipsam.*

The quotation from Vossius translates as: ‘The year 1130 Athelard, or Adelard, an Englishman, a monk of Bath, translated the geometry of Euclid from Arabic to Latin. Nor is it any wonder he knew Arabic when he had been not only to France, Germany and Italy but also to Spain, Egypt and Arabia itself.’ Modern scholarship has modified this account: Adelard (c.1080-1150) travelled widely in France, Sicily (before 1116), Cilicia (in what is now eastern Turkey), Syria and Palestine but there is no firm evidence that he ever visited Spain.<sup>72</sup> He is best known for the first translations of Euclid from Arabic to Latin, and three versions are ascribed to him.<sup>73</sup> Wallis knew one version in the Savile Library and another in Trinity College.<sup>74</sup> Adelard also translated the astronomical tables of al-Khwārizmī, *Ezich elkaurizmi*; the Bodleian Library owns a copy that is richly and beautifully illustrated in red, green and gold.<sup>75</sup>

§ 2.17 And *Robertus Retinensis* (*Robert of Reading*) who travelling into *Spain* on the account of the *Mathematics*, did there translate the Alcoran out of *Arabic* into *Latin*, in the year 1143. (as appears by his Epilogue to that Translation, and the Preface of *Petrus Cluniacensis* thereunto.)

There is no mention of Robert in the pages of *De scientiis* and his inclusion here is a result of Wallis’s own researches among Bodleian manuscripts: a copy of Robert’s translation of the *Koran*, made for Peter, Abbot of Cluny, was acquired by the library as part of the Selden collection.<sup>76</sup> In the preface (which appears in the Bodleian manuscript as a colophon) Robert wrote that

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<sup>72</sup> Burnett 1987.

<sup>73</sup> Adelard I is a close translation of the entire work; Adelard II was the most popular version but omits many of the proofs; Adelard III is a commentary rather than a translation. Recent scholarship has questioned the true authorship of Adelard II and has suggested that it should be ascribed to Robert of Chester, see Busard and Folkerts 1992.

<sup>74</sup> MS Savile 19 (Adelard II); MS Trinity College 47 (Adelard I).

<sup>75</sup> *Ezich elkaurizmi*, MS Auct F.19.

<sup>76</sup> MS Selden Supra 31, ff. 32-204. Peter commissioned the translation so that he could refute Islam, see Migne CLXXXIX, col 649f; col. 659f.

he now intended to return to his chief interest, mathematics,<sup>77</sup> and for Wallis this was reason enough to count him among the English translators of Arab learning.

Robert's name actually appears at the end of the translation as *Ketenensis* but the looped 'K' was read by Wallis as 'R'. Robert was in fact *Robert of Chester* whose name has mutated through the forms Cestrensis, Kestrensis, Ketenensis and Retinensis leading to confusion which persists to the present day: the Bodleian Library catalogue entry for the manuscript describes the author as 'probably of Ketton in Rutland' (whereas Wallis translated 'Retinensis' as 'of Reading'). The *Dictionary of national biography* still carries two articles on Robert, headed 'Chester, Robert (fl 1182)' and 'Robert the Englishman, (de Ketenes, de Retines) (fl. 1143)'. The date 1182 in the former arises from the dating system then in use in Spain; it was in fact the year we would now denote as 1144, which at least brings the two Roberts into the same time frame.

Little is known of Robert's life. He was in Spain from about 1140 and lived near the river Ebro in the north east. He worked closely with another translator, Hermann of Carinthia, who appears to have come from the region that borders modern Austria and Slovenia (he is also sometimes known as Herman of Dalmatia), which suggests a trans-European dimension to the translation programme that Wallis either failed to see or chose to ignore.<sup>78</sup> As a translator Robert was far more important than Wallis knew. By misreading his surname Wallis failed to recognise him as the translator of the *Canons* of Arzachel (to be discussed in § 4.6). Robert is now also thought to be the possible writer of 'Adelard II'.<sup>79</sup> He is best known, however, for the first Latin translation of al-Khwārizmī's *Al-jabr wa'l muqābala*, the key text in the evolution of Arabic and European algebra. No copy of Robert's translation

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<sup>77</sup> *Prefacio Roberti translatoris*, MS Selden Supra 31, ff. 32-33; Migne CLXXXIX, col 657 f.

<sup>78</sup> Thorndike 1923, II, 14-98, 155-187; Haskins 1924, 3-66, 113, 129; Rose 1975, 76-89; Lindberg 1978, 52-90.

<sup>79</sup> See note 73.

reached England,<sup>80</sup> and unfortunately Wallis never knew of this important English contribution to the early development of algebra.

§ 2.18 About the same time (or somewhat sooner) *Guilielmus de Conchis* (*William Shelley*) is said to have travelled into *Spain* to furnish himself with *Arabic* and *Mathematical* Learning; and brought from thence divers *Arabic* Books.

*Guillaume de Conches* (d. ?1154) was a natural philosopher, born in Normandy.<sup>81</sup> He studied at Chartres, taught at Chartres or Paris, and retired to Anjou where he wrote the philosophical work for which he is best known, his *Dragmaticon*. There is no evidence that he ever went to Spain, nor that he was familiar with Arabic language or philosophy, or with astronomical tables of any kind. Nor was he mentioned by Vossius. His inclusion by Wallis is therefore puzzling until we look at the next name, Daniel Morley.

§ 2.19 And, soon after, *Daniel Merlacus* (*Morley*), about the year 1180 made several Journeys into *Spain* on the like account, where (at *Toledo*) *Arabic* and *Mathematical* Learning were in great request (brought thither by the *Moors*) which in other parts of *Europe* were scarce known. And these brought with them that kind of Learning into *England* very early, with store of *Arabic* Books.

This information about *Daniel Morley* (fl. 1170-1190) is not to be found in *De scientiis*, but in the preface<sup>82</sup> to Morley's only work, his *Liber de naturis inferiorum et superiorum*. There Morley helpfully gave a brief account of his life and travels which tallies with the summary given by Wallis. First, he said, he went to Paris but found that the teachers there carried only 'leaden pens' with which they marked asterisks and obelisks reverently in their texts, so he went on south to Toledo in search of something better, in particular the contents of the *quadrivium*, the four Classical branches of mathematics. On returning to England with a good collection of books, he was depressed by the neglect of Plato and Aristotle there, and decided to return to Spain, but was waylaid by John, Bishop of Norwich (1175-1200), for whom he wrote his treatise. The only manuscript copy of the preface now surviving is in the

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<sup>80</sup> Karpinski 1915, 49-63; Hughes 1982.

<sup>81</sup> It was the English historian John Bale (1506-1552) who claimed that Conches was born in Cornwall and who introduced the anglicised form of his name, Shelley.

<sup>82</sup> Reprinted in Halliwell 1839, 84-85.

British Library,<sup>83</sup> but Wallis knew of another copy that had been in Oxford a few years earlier:

§ 2.20 A particular account of these Travels of *Shelley* and *Morley* was a while since to be seen in two Prefaces, to two Manuscript Books of theirs in the Library of *Corpus-Christi* College in *Oxford*, but hath lately (by some unknown hand) been cut out, and carried away; which Prefaces (one or both of them) did also make mention of the Travels of *Athelardus Bathoniensis*, and are, to that purpose, cited by *Vossius* out of the Manuscript Copy. Who ever hath them, would do a kindness (by some way or other) to restore them, or at least a Copy of them.

The Corpus Christi manuscript to which Wallis referred is that now known as MS CCC 95. It includes a copy of the *Liber de naturis* from which the preface has, as Wallis described, been neatly cut out, but the contents page lists the opening work as *Philosophia magistri Daniel de Merlac*. Morley's book is followed without a break by the *Dragmaticon* of de Conches, which ends 'Explicit Will de Conchys', an attribution which led to a mistaken identification in Henry Coxe's 1852 catalogue.<sup>84</sup> Wallis (or his unknown informant) must have been similarly misled, and Wallis assumed that the missing preface described a journey taken by both men, even though he dated them forty years apart.

The preface relating to Adelard comes from a different Corpus Christi manuscript, MS CCC 86, containing Adelard's *De causis*.<sup>85</sup> It was used by

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<sup>83</sup> Sudhoff 1918; Birkenmajer 1970, 45-51.

<sup>84</sup> The manuscript is described by Coxe 1852 as 'three books of the Norman philosopher William de Conches, alias Shelley'. The error was pointed out by H. Nash in a letter to Corpus Christi librarian, Charles Plummer, written 25 March 1889. The letter (preserved with MS CCC 95) begins: 'I have been to see the BM MS (Arundel 377) of Daniel de Morley. It is the same book as the one in your library and it is then also followed by a dialogue between the Duke of Normandy (D) and the Philosopher (P) of Gul. De Conchis. Coxe confounded the two. A passage which I copied from your MS fol 15b occurs on the last folio of the Arundel 377, where the division between the two is quite distinct. You will see the 'incipit' of Gul de Conchis in the Arundel catalogue. I mention this as you may like to make a note of the fact in your copy of Coxe's catalogue. The beginning (missing in the CCC MS) contains a delightful little piece of autobiography.'

<sup>85</sup> Adelard, *De causis naturalium compositorum*, MS CCC 86, f. 163.

Vossius to date Adelard's activities to 1130, and Wallis would have known of it from the reference in *De scientiis*.<sup>86</sup>

§ 2.21 About the same time were *Johannes Sarisburiensis*, *Rogerus Infans*, and divers others of the *English*.

*John of Salisbury* (d. 1180) was one of the best known scholars of the day. He travelled as far as southern Italy, but knew little Greek and no Arabic, and employed an Italian Greek to make translations of Aristotle. He was primarily a theologian and no lover of mathematics, which to him meant astrology: in his *Polycraticus* he defined mathematicians as those 'who from the position of the stars and the motion of the planets foretell the future', and classed mathematics with chiromancy, sortilege and augury as one of the magic arts, and a source of evil.<sup>87</sup>

*Roger Infans* was the scholar *Roger of Hereford* (fl.1178), but Wallis never used the second, more usual, form of his name. Roger was a natural philosopher, computist and astrologer, with special knowledge of mines and minerals, and was familiar with some Arabic texts, but it is not known whether he made his own translations.<sup>88</sup> There is no mention of him in *De scientiis* and Wallis must have come across the unique occurrence of 'Infans' in MS Digby 40, one of the few instances where we can be sure that Wallis consulted the Digby collection. Roger's *Tractatus de computo* in MS Digby 40 is headed 'Tractatus Rogeri Infantis', apparently because Roger said that he wrote it while still a young man. As a result, he, like Robert of Chester, has acquired two entries in the *Dictionary of national biography*: 'Roger Infans (fl.1124)' and 'Roger of Hereford (fl.1178)'. The mismatching dates stem from the figure 1124 which appears in the margin of the *Tractatus de computo*, but which was meant as part of the calculation, not as a date of writing. Roger's name was anglicised by the historian John Leland (1506-1522) to 'Yonge'; Wallis in the 1693 Latin translation of *A treatise of algebra* went further and gave his name as 'Roger Child'.<sup>89</sup>

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<sup>86</sup> Vossius 1650, 176.

<sup>87</sup> Migne CIC, cols 407-409.

<sup>88</sup> Russell 1932; French 1996.

<sup>89</sup> Wallis 1693, 6.



§ 2.22 Before these times the *Arabic* Language, and *Greek* it self, being but little known in these Parts, Mathematical Learning was but very rare, and slenderly improved in *Europe*. We had indeed in *England*, *Althelmus* or *Adelmus*, whom Vossius placeth about the year 680; and *Walfridus Ripponensis*, placed by him at 690; and *Bede* (the most eminent of that Age) at 730; and *Albinus* or *Alcuinus*, (a Scholar of *Bede*) at 760; but *Euclid* and *Ptolemy* were unknown to them, *Boethius* and *St. Augustin* being their most Classic Authors for such Learning.

Wallis recognised that during the period when Greek was lost in Europe and Arabic not yet understood, there was little mathematical learning of any significance. *Boethius* (480-524 AD), who witnessed the death throes of the Roman empire in the west, based his *Arithmetica* on the earlier *Introductio arithmeticae* of Nicomachus (c.100 AD), essentially a treatise on Pythagorean number relationships. As a mathematician Boethius was no more than a pale shadow of the great Classical writers, but in an age when, as Wallis described, Euclid and Ptolemy were almost completely lost, he was one of the few remaining links to the Greek mathematical past and his *Arithmetica* was copied and used for centuries.<sup>90</sup>

It is more difficult to justify the inclusion of *St Augustine* as an upholder of Classical mathematics. Wallis, however, annotated his copy of *De scientiis* with a reminder of the use of mathematics in theology and quoted Augustine: '*nemo ad rerum divinarum, humanorumque, cognitionem accedat, nisi prius numerandi artem addiscat*', ('no one can attain knowledge of things divine or human unless he first learns also the art of numbering').<sup>91</sup> In *The city of God* Augustine argued that the science of number was an aid to interpretation of the scriptures, and speculated that the universe was created in six days because six is a perfect number.<sup>92</sup> It seems, though, that Wallis had something more practical in mind for he referred to the use of mathematics in the calculation of chronology. More generally, the correct measurement and division of time, an art known as *computus*, was extremely important in a society increasingly concerned with the correct regulation of religious life and festivals, and served to keep some advanced arithmetic alive during the early medieval era. Three

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<sup>90</sup> Evans G.R. 1978; Masi 1983; Oosthuit and Schilling 1999.

<sup>91</sup> Savile G.21, 30-31.

<sup>92</sup> Augustine, *De civitate dei*, Book 11, Chapter 30; Migne XLI, cols 345-346.

of the four English scholars mentioned by Wallis (Aldhelm, Wilfrid and Bede) were renowned computists.

*Aldhelm* (640-709), Abbot of Malmesbury and Bishop of Sherborne, was educated at Malmesbury and Canterbury in law, computation and astronomy, and wrote sophisticated Latin.<sup>93</sup> He was the author of *Liber de septenario*, a treatise on the number seven, but it was a mystical rather than mathematical work; his reputation for mathematics arose not from this but from the quarrel between the Celtic and Roman churches over the calculation of the date of Easter in which Aldhelm was a proponent of the Roman method, based on the 19 year lunar cycle. His exact contemporary, *Wilfrid of Ripon* (634-709), Archbishop of York, was instrumental in getting the Roman method accepted at the synod of Whitby in 664.<sup>94</sup>

*Bede* (672-735) was by far the most prolific scholar of the period.<sup>95</sup> He spent all his life at the monastery of Jarrow-on-Tyne, Northumberland, which for a brief time was a focus of learning collected from Ireland, continental Europe and even north Africa. Most of Bede's writing was on theology and history, but he also wrote a *Computus*.<sup>96</sup> Bede's work became known on the continent through his pupil *Alcuin* (or *Albinus*) (735-804) who became an adviser to Charlemagne, and was the fourth of the scholars mentioned by Vossius and Wallis.<sup>97</sup> Alcuin encouraged the study of mathematics and the computus, and is often credited with a set of 53 arithmetic and geometric puzzles, the 'Propositions for sharpening the minds of youth'.<sup>98</sup>

Thanks to Alcuin, Bede's influence survived longer in continental Europe than it did in Britain. (The best manuscript of Bede's *Computus* in the Bodleian Library comes not from England but from France.<sup>99</sup>) In England,

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<sup>93</sup> Vossius 1650, 171, 312, 395; Migne LXXXIX.

<sup>94</sup> Vossius 1650, 395; Migne XCV.

<sup>95</sup> Vossius 1650, 171, 312; Migne XC. For an assessment of Bede's mathematics see Jones C. 1970.

<sup>96</sup> Migne XC, cols. 277f, 293f.

<sup>97</sup> Vossius 1650, 171; Migne C, CI.

<sup>98</sup> Migne CI, cols. 1143-1160; Folkerts 1978; Singmaster and Hadley 1992.

<sup>99</sup> MS Bodl. 309, ff. 3<sup>v</sup>-62, 68-80, from the Abbey of the Holy Trinity, Vendome, France c. 1075.

Bede's learning was never more than a fragile candle in a vast surrounding darkness, and it was all but extinguished in the invasions and instability of the three following centuries. Only early in the twelfth century did scholars in England and elsewhere across Europe become aware of the knowledge that all this time had been accumulating in Islamic Spain, and some of the more adventurous travelled south and brought back texts that were to set the intellectual life of northern Europe on a new course.

§ 2.23 But after these times, having received from the *Arabs* divers Translations of *Euclid*, *Ptolemy*, *Aristotle* and other *Greek* Authors, with divers improvements in Philosophy, Astronomy, Geometry and other parts of Mathematics, these Studies were strangely advanced, and especially in England, where (beside those above mentioned) we had *Clement Langthon*, whom *Vossius* placeth about 1170; *Gervasius Tilburiensis*, about 1210; *Johannes de Sacro Bosco*, about 1232; *Robertus Lincolniensis* (*Robert Grosthead*) about the same time; *Roger Bacon*, about 1255; *Johannes Peccam* (or *Johannes Cantauriensis*) about 1276; *Odingtonus*, about 1280; *Johannes Bacondorpius*, about 1330; *Robert Holcot* (or *de Northamptona*) about 1340; *Johannes Estwood* (*de Ashenden*), about 1347; *Climitonus Langley*, about 1350; *Nicolaus Linnensis*, about 1355; *John Killingworth*, about 1360; *Richard Lavingham*, about 1370; *Simon Bredon*, about 1386; *John Sommer*, about 1390; *John Walter*, about 1400; *William Batecombe*, about 1410; *William Buttoner*, about 1460; who were, many of them, very eminent, as in other kinds of Learning, so particularly in the Mathematics; and divers of their Works are extant in our Libraries, which have not yet been printed.

§ 2.24 Besides others whom *Vossius* mentions not: As *Adamus de Marisco* (*Adam Marsh*), contemporary with *Grosthead* Bishop of *Lincoln*, intimate with him, and commended by him; *Bradwardine* and *Read*, and divers others about that Age.

Wallis began with a generous recognition that the Arabs had not only preserved and translated the Classical heritage but had developed and improved upon it, a sign of Oxford's new respect for Arabic learning, in contrast to European attitudes in earlier centuries.<sup>100</sup> The influx of new texts from Arabic had revolutionised learning throughout western Europe, but Wallis was concerned only with England, and justified his claim that mathematical studies moved forward 'especially in England' by producing a long list of thirteenth- and fourteenth-century English 'mathematicians.' The list was compiled by the same method Wallis had used for his Arab writers, by combing the pages of *De scientiis* for the names of every English writer he

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<sup>100</sup> Rose 1975, 262-263; Moyer 1999, 480-481.

could find and then arranging them in chronological order (according to the dates given by Vossius). Vossius in turn had gathered his information on these writers from the earlier researches of the English historians John Leland, John Bale and John Pits, all of them assiduous collectors of information on medieval writers and manuscripts (see Appendix II: English sources used by Vossius).

Wallis was indirectly, therefore, using the best available evidence of the time, much of it collected during the sixteenth century from the libraries of Oxford, Cambridge, London and Norwich, and from the monasteries at the time of their dissolution. To the modern reader, however, the list is a curious mixture of names well-known and obscure, with widely varying claims to mathematical prowess.<sup>101</sup> Langthorn, Tilbury and Lavenham would hardly have thought of themselves as skilled mathematicians and are certainly not so remembered now. On the other hand there are some surprising omissions. Henry Savile in 1570 had classed the medieval mathematicians Richard Swineshead, Roger Bacon and Richard Wallingford on a par with Archimedes and Ptolemy<sup>102</sup> but by the seventeenth century Swineshead and Wallingford had slipped into oblivion.

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<sup>101</sup> In their modern forms the names in Wallis's list at § 2.24 are: Clemens Langthorn, Gervase of Tilbury, Johannes Sacrobosco, Robert Grosseteste, Roger Bacon, John Pecham, Walter Odington, John Baconthorpe, Robert Holcot, John Ashenden, Richard of Kilvington, Nicholas of Lynn, John Killingworth, Richard Lavenham, Simon Bredon, John Somer, John Walter, William Batecombe, William of Worcester or Botoner, and at § 2.25: Adam Marsh, Thomas Bradwardine, William Rede. For dates, biographies and bibliographies see Emden 1957; Kretzmann, Kenny and Pinborg 1982, 853-892; Sharpe 1997. References that have been found useful include: Pedersen O. 1985 (Sacrobosco); Thomson 1940, Hunt 1955, Clanchy 1979, Southern 1986 (Grosseteste); North 1976, III, 238-270 (Odington); Xiberta 1927; North 1992b, 105-106 (Baconthorpe); Smalley 1956, Thorndike 1957, Tachau 1995 (Holcot); Snedegar 1988 (Ashenden); Kretzmann 1990 (Kilvington); North 1988, 87-133 (Lynn and Somer); North 1989a, 343-346; North 1992b, 124-127 (Killingworth); Talbot 1962 (Bredon); North 1986, 126-130 (Walter); North 1989a, 337-342 (Batecombe); North 1986, 186-195 (Botoner); Clagett 1959, 220-222, 230-234; North 1992a, 79-82 (Bradwardine); North 1989a, 332-336 (Rede).

<sup>102</sup> MS Savile 29, f. 3<sup>v</sup>.

Wallis's omission of Swineshead is particularly unaccountable since he knew something about him: Vossius in *De scientiis* described him as 'Ioannes Suisser . . . vulgo dictus Calculator', and Wallis corrected this entry to 'Raimundus Suisset', the name he would have known from the 1520 edition of Swineshead's *Calculationes* in the Savile Library.<sup>103</sup> Wallis also knew that Cardano had placed Suisset fourth in his list of great scientists (after Archimedes, Aristotle and Euclid but ahead of Apollonius),<sup>104</sup> but despite this tribute to England from Italy, Wallis failed to include Suisset under any of the variations of his name. Richard of Wallingford has been described as 'perhaps the best mathematician and astronomer of the Middle Ages',<sup>105</sup> and his *Tractatus de sinibus demonstratis* survives in three copies in the Digby collection (in MSS Digby 168, 178, 190). Vossius omitted both Wallingford and the astronomer John Maudith, though both were recorded by Bale in his notebook and in his 1557-59 *Catalogus* where they appear as 'Ricardus Vualingforde' and 'Ioannes Manduith'.<sup>106</sup> They were absent, however, from Bale's 1548 *Summarium*, which was therefore probably the edition used by Vossius. Since they escaped the attention of Vossius they were also missed by Wallis. The omission of Swineshead and Wallingford suggests that Savile's eulogy, though available to Wallis in the Savile Library, was also unknown to him.

Some of the writers mentioned by Wallis will be discussed in greater detail later in relation to Wallis's Chapter 4, but two of them, John Ashenden and Robert Holcot, will be given special mention here because Wallis himself singled them out for extra research.

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<sup>103</sup> Swineshead 1520 (Savile X.6), 74; Vossius 1650 (Savile G.21), 5. Richard Swineshead was variously known as 'Suuinsete', 'Suiseth' or 'Suisset'. He was not always distinguished from his contemporaries Roger and John Swineshead, so that his first name sometimes appears as Ioannes, Rudiger, Reyner or Raimundus. In Swineshead 1520, his name is given as *Ricardus* in the title but *Raimundus* in the colophon.

<sup>104</sup> Swineshead became better known in Italy than in England: his *Calculationes* was published at Padua c.1477, Pavia 1498 and Venice 1520. See also Clagett 1959, 290-304; North 1992a, 89-92; Molland forthcoming (b).

<sup>105</sup> North 1999, 33; North 1976.

<sup>106</sup> Bale 1557-59, 397, 426.

§ 2.25 That of *John Estwood* (or *Estwyde*, or *Eshwood*, or *Eshwid*, or *Eschuyde*,) *de Ashenden*, (or *Eshenden*, or *Ashenton*, or *Aysden*, for so many ways I find it written) I find printed at *Venice*, in the year 1489, under the name of *Summa Astrologiae Judicialis de Accidentibus mundi, quae Anglicana vulgo nuncupatur, Joannis Eschuidi viri Anglici, peritissimi scientiae Astrologiae*; (which I mention, because his printed name differs so much from the manuscripts.) And (for the age of it) in two ancient Manuscript Copies, I find it thus subscribed, *Completa est haec compilatio tractatus secundi summae Judicialis de Accidentibus Mundi, 18 die mensis Septembris, Anno Christi 1348*, (which I take to be the Author's own words.) And then follows, *Explicit summa Judicialis de Accidentibus Mundi secundum magistrum Johannem de Estemdene, quondam socium Aulae de Merton in Oxonia*. The one of these manuscripts is in the *Bodleian Library*, the other in the *Savilian*.

John Ashenden was considered one of the great medieval astronomers:<sup>107</sup> his works survive in many manuscripts in the Savile, Digby, Selden and Ashmole collections and his *Summa astrologiae judicialis de accidentibus mundi* ('A summary of the judgements of astrology on the happenings of the world') was indeed printed at Venice in 1489.<sup>108</sup> It is not surprising that the various forms of 'Eastwood' and 'Ashenden' caught Wallis's attention: Emden, in his *Biographical register of the University of Oxford* listed five additional variations of 'Eastwood' and no fewer than twenty-four of 'Ashenden'. Even this list is incomplete as I have found several further spellings of Ashenden not listed by either Emden or Wallis. I have attempted to correlate Wallis's spellings with those to be found in the manuscripts in order to trace his sources, but without any great success. The best identifications are the unique forms 'Essomdene' (or 'Estomdene') in the colophon to the second book of Ashenden's *Summa judicialis* in MS Savile 25, and 'Aysden' in a later hand in the same manuscript. MS Savile 25 would be the Savilian manuscript identified by Wallis:<sup>109</sup> it contains the second book (only) of Ashenden's *Summa judicialis* and the colophon is as quoted.<sup>110</sup> The manuscript Wallis

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<sup>107</sup> Snedegar 1988.

<sup>108</sup> Ashenden 1489. The copy known to Wallis was probably that in MS Ashmole 576.

<sup>109</sup> During the seventeenth century the Savile collection was held separately from the main Bodleian collections. It was not incorporated into the Bodleian Library until the nineteenth century.

<sup>110</sup> MS Savile 25, ff. 1-63. The colophon translates as: 'This compilation of the second book of the summary of teachings was completed the 18<sup>th</sup> day of September, year of Christ 1348.

knew in the Bodleian Library is harder to identify. There are two possibilities: MS Bodl. 369 (acquired in 1607) and MS Bodl. 714 (acquired from Thomas Allen in 1601). Both are complete copies of the *Summa judicialis* and end with the colophon already cited, with the names ‘Esshenden’ and ‘Eschenden’ respectively. Wallis could have seen either. The form ‘Esshenden’ corresponding to Wallis’s ‘Eshenden’ appears in its most unambiguous form in MS Bodl. 369 at f. 379<sup>v</sup>. There are also copies of the *Summa judicialis* in MS Digby 159 and 225 but Wallis appears not to have considered the Digby collection as ‘Bodleian’ manuscripts (see § 4.10 below).

§ 2.26 And I guess, that *Robertus de Holcot* (mentioned by *Vossius*), and *Robertus de Northampton*, (of whom, in the *Savilian* Library, we have some mathematical Tracts in MS) might be the same person, (but am not sure of it,) because I find (in the County of *Northampton*) a Village called *Holcot* (about five miles distant from the Town of *Northampton*, Northward), and another called *Hulcot* (about as far Southward from *Northampton*), where, within a few years last past (as I am told by one who knew the person) lived one of that name (*Hulcot* of *Hulcot*) whose ancestors had lived there for a long time; (from some of whom perhaps that place might take the name, or they from it.) Now both of these places being near to the Town of *Northampton*, and within the County, it’s not at all unlikely, that (in those days, when, for want of Surnames, Men were wont to be distinguished from the places of their Birth, or of their Abode) the same person might be indifferently called *Robertus de Holcot*, (*Hulkot*, or *Holkoth*,) and *Robertus de Northampton*.

Wallis was right to remain cautious about identifying Robert of Northampton with Robert Holcot. Robert of Northampton wrote an explanation, now in MS Savile 21 (ff. 42-61<sup>v</sup>), of the *Theorica planetarum* of Roger of Hereford (see § 2.21). Robert Holcot, on the other hand, was famous for 200 years for his biblical commentaries, but not for mathematics. However, Bale ascribed to Holcot a work called *De effectionibus stellarum* (‘The effects of the stars’).<sup>111</sup> This treatise has since been discovered, and is theological rather than mathematical,<sup>112</sup> but the title alone was sufficient for Vossius (and hence Wallis) to regard Holcot as an astronomer.

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Here is set forth a summary of the teachings on the happenings of the world according to master John Estomdene, sometime fellow of Merton at Oxford.’

<sup>111</sup> Bale 1548, 148.

<sup>112</sup> Thorndike 1957; Tachau 1995, 255-265.

Robert Holcot was certainly associated with Northampton:<sup>113</sup> he lived at the Dominican convent in the city from 1343 until his death from plague in 1349, and probably came originally from the village of Holcot five miles to the north-east (the church there still contains wall paintings from the second half of the fourteenth century). Vossius gave the date of Robert Holcot's death erroneously as 1376, so he too may have identified him with Robert of Northampton. Place names, as Wallis realised, can be a useful guide to medieval identity but in this case may have confused the issue. The second village, Hulcot, where Wallis made his enquiries, is now absorbed into Northampton itself. Wallis had family connections in this part of Northamptonshire<sup>114</sup> and his research in the area shows how far he took his interests beyond the confines of Oxford and its libraries.

§ Chapter 3. Of the Numeral Figures now in use, from whence we had them  
Chapter 3 marks a distinct change in Wallis's style and method. From his sweeping overview of Greek, Arab and English mathematics he now moved into a detailed study of a single topic: the development of the modern numeral system. This was a theme that was to occupy him in one way or another for the next ten chapters ending with the latest advances, the development of decimal fractions and logarithms. But it was here in Chapters 3 and 4 that Wallis did some of his best research, into the origin and spread of the Hindu-Arabic numerals.

§ 3.1 Amongst the Improvements in Mathematics (and particularly in Arithmetic), which we received from the *Moors* and *Arabs*, that of the *Numeral Figures*, which we now use, is very considerable: Ten in number; 1, 2, 3, 4, 5, 6, 7, 8, 9, 0.

§ 3.2 Which though they be not just the same with those of the *Arabic*, yet they are, most of them, so little different from them, that it cannot be doubted but that our Figures are derived from theirs. And those of former times (when these Figures came first into use) were yet more like to the Arabic Figures, than those we now use, which, in process of time, are by little and little sensibly varied from what at first they were: As is manifest, if we compare those we now use, with those which were then used when

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<sup>113</sup> Smalley 1956, 7-9.

<sup>114</sup> Wallis's daughter Anne married John Blencow of Marston St. Lawrence in December 1675. Blencow is still a familiar name in the area. His second daughter Elizabeth married William Benson of Towcester, in 1682.



Printing first came in; and much more if compared with those of ancient Manuscripts before Printing

In Chapter 2 Wallis might have been content with such general statements, but now he began to support his claims with detailed evidence.

§ 3.3 And those of *Maximus Planudes*, (whom *Vossius* placeth about the year 1370; but *Kircher* in his *Arithmologia* thinks him to have lived about 1270, and to have dedicated some of his Works to the Emperor *Michael Palaeologus*) are almost just the same with those of the *Arabs*; of whose Arithmetick, in *Greek*, we have two Manuscript Copies in the *Bodleian Library*.

Maximus Planudes (c.1255-1310) was a Greek monk who travelled as an ambassador between Constantinople and Venice, and was a prolific translator from Greek to Latin. He wrote a commentary to Books I and II of Diophantus of Alexandria, a partial copy of which survives among the Savile manuscripts.<sup>115</sup> The 'Arithmetick' of Planudes was his *Ψηφοφορία κατὰ Ἰνδοῦς* ('Indian calculation') which taught the Indian figures and methods of calculation.<sup>116</sup> Wallis was correct in observing that there were two copies of Planudes' *Ψηφοφορία* in the Bodleian Library: MS Gr. Laud 51 and MS Cromw.12, the gifts of William Laud and Oliver Cromwell respectively. There were ironies here that Wallis can hardly have failed to notice. Laud had been executed in 1645 during the war from which Cromwell emerged triumphant. The two men stood on opposing sides of the fundamental religious and political schisms which divided England in the 1640s, but both in their turn were Chancellors of Oxford University, and their names are now engraved next to each other on the great marble slab commemorating the Bodleian Library's benefactors. It is to these two men that the Library owes its two copies of the *Ψηφοφορία*.

Vossius' date for Maximus Planudes is rather too late, and Wallis checked it from another source, the *Arithmologia* of the Jesuit writer Athanasius Kircher.<sup>117</sup> Kircher's book was essentially on the magic and arcane properties

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<sup>115</sup> MS Savile 6, ff. 91-106 is a copy of Planudes' commentary on Diophantus up to Book I.16. The full commentary was first published in Diophantus 1575.

<sup>116</sup> The Greek word *Ψηφος* means 'pebble', the equivalent of *calculus* in Latin. Planudes' text does not appear to have been translated into English. For the Greek text and a German translation see Planudes 1865. For a French translation see Planudes 1981.

<sup>117</sup> Kircher 1665, 44-47.

of numbers but he, like Wallis, was interested in how and when the numerals had reached northern Europe, and he too identified Planudes as a source but placed him in the reign of emperor Michael III Palaeologus (d.1282) and so dated him correctly at about 1270. Kircher's opinions on the routes and dates of transmission of the numeral system will be discussed further below.

§ 3.4 But when I speak of those Figures as brought to us from the *Arabs*, I do not so much mean those very Characters which we now use, (though it be true of them also) as of the way of Computation by them; each of them, beside their own particular value, receiving a several Denomination, according as they stand in the first, second, or third place, and so forth, as far as occasion serves, each place exceeding that below it in Decuple proportion; and then, whether we retain just the same Figures, or others somewhat varied from them, (according as the fashion of letters in divers Countries, and divers Ages, do use to vary,) it is much one.

Here Wallis made an important point: that the real advance was not in the Hindu-Arabic symbols but in the system of place-value introduced with them, 'the way of computation', with its unprecedented computational power and flexibility.

§ 3.5 Before these Figures were introduced, while we had no other ways of Notation for Numbers than that of the *Latin*, by a few Numeral Letters, M D C L X V I; or of the *Greeks* by the Letters of the Alphabet,  $\alpha$ ,  $\beta$ ,  $\gamma$ ,  $\delta$ , &c. (like as before them, the *Hebrews*, *Arabs*, and other Orientals, did also design Numbers by the Letters of their Alphabet:) The exercise of Practical Arithmetic, especially in large Numbers, was but very lame, in comparison of what now it is.

It is another sign of seventeenth-century Oxford's new strength in oriental studies that Wallis was familiar not only with Classical but with Arab, Hebrew and other non-western sources. As early as 1657 he had already discussed alphabetic numeral systems in Hebrew, Greek and Latin with references to Arabic, Persian, Turkish and even Chinese.<sup>118</sup> All alphabetic systems were unwieldy for carrying out large or complex calculations but were nevertheless used successfully for hundreds of years both for recording and for basic calculations. Wallis went on to give three examples of calculations in alphabetic numerals from Greece and medieval Europe:

§ 3.6 As will appear very evident, if we look into *Eutocius* (in his Commentary on *Archimedes*, *De dimensione Circuli*), or other of the

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<sup>118</sup> Wallis 1657b, Chapters 7-8. For alphabetic numeral systems in Hebrew, Greek and Arabic and others derived from them see Ifrah 1998, 212-247.

Ancients, to see how troublesom a thing it was with them to multiply, divide, or extract the Root of a large Number.

At about the time he was writing *A treatise of algebra* Wallis was also engaged in the publication of two works of Archimedes, the *Arenarius* and the *Dimensio circuli*, both of which he compiled and corrected in Greek and Latin from earlier editions.<sup>119</sup> Wallis's version of *Dimensio circuli* included the commentary of Eutocius (c.560 AD) who remarked on the difficulty of Archimedes' calculations with fractions and square roots.<sup>120</sup> The difficulty is largely inherent in the calculations themselves but can only have been exacerbated by the shortcomings of the notation then available.

§ 3.7 And so likewise in *Bede*, or others, to see what perplex Rules they are fain to give in these cases, which are now dispatched with a great deal of ease.

The Bodleian Library now owns about eighty manuscripts of works by Bede, but most are theological and I have discovered only one that contains calculations, MS Bodl. 309, already referred to in § 2.22. The volume opens with Bede's *De ratione temporum* which described the 'nature, course and end of time' and included a 532-year table of Easters (28 x 19-year cycles). The volume continues in the same hand with a calendar of events, followed by part of the *Arithmetica* of Boethius, but the latter starts in mid sentence at Book I, Chapter 16. No author or title is named but Wallis would almost certainly have recognised the work and would no doubt have noted the multiplication square for 1 to 10 in Roman numerals.

§ 3.8 And the like in a Fragment we have in Manuscript of the *Second Book of Pappus's Collections*, which is all employed in Rules for the Practice of Multiplication of great Numbers, much like those of *Bede*.

Book I and the first thirteen propositions of Book II of the *Mathematical collections* of Pappus (c.320 AD) are lost, but a copy of the second part of Book II is in the Savile Library and was edited and published by Wallis in 1688.<sup>121</sup> In it Pappus reproduced the methods of Apollonius (c.225 BC) for

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<sup>119</sup> Archimedes 1676. Wallis drew on the earlier editions of Geschauff [Ventorius], Commandino and Rivalentus.

<sup>120</sup> Heath 1931, 305-309.

<sup>121</sup> MS Savile 9, ff. 41-48; Wallis 1688b. Jones A. 1986, 46-47, suggests that Book I is extant in Arabic. Books III-VIII were first translated and published by Commandino in 1588.

multiplying large numbers; he stated, for example, that  $500 \times 40$  was equivalent to  $(5 \times 4) \times 1000$ , a fact not immediately obvious in an alphabetic system.

§ 3.9 Or if, without consulting those Authors, we do but consider which way we should go about first to design, and then to extract the Square or Cubic Root of a Number to ten or twenty places (as we now design it), if we had no other way to express it, than by those Numeral Letters, M D C L X V I.

The modern description of a number as having ten or twenty decimal places is itself, as Wallis pointed out, a positional concept. For all the benefits of the modern number system, however, few would relish the task of calculating square roots to such a degree of accuracy without mechanical aids. Not so Wallis who wrote to Thomas Smith that on 22 December 1669: 'In the dark night in bed I did extract the square root of 3, 00000, 00000, 00000, 00000, 00000, 00000, 00000, 00000 which I so found to be 1,73205, 08075, 68877, 29353 &c. And did next day commit to writing.'<sup>122</sup> Although Wallis wrote this some twelve years after the event there is sufficient evidence elsewhere of his prodigious powers of calculation to lend the story some credence.

§ 3.10 'Tis true, the *Arabs* had, and yet have, a way of expressing small Numbers (in like manner as the *Greeks* or *Hebrews*) by Letters of the Alphabet. And herein they follow the order of the *Hebrew* Alphabet; which I therefore think was anciently the order also of the *Arabic* Alphabet, though later Grammarians (for putting those Letters together, whose Figures are like; and differ but in Diacritical Points) have now disposed the *Arabic* Letters in another order.

Wallis was correct in identifying the Arabic alphabetic numerals with the Hebrew equivalents, and made interesting use of this mathematical information to argue (also correctly) about the history of the Arabic alphabet. Both the Hebrew and Arabic alphabets, like almost every other alphabet now in use, were derived from the Phoenician alphabet devised in the fifteenth century BC. The order of the twenty-two Phoenician letters was fixed as early as the fourteenth century BC, and although extra letters were sometimes interspersed in other languages, the original order has remained more or less unchanged in nearly all later alphabets.<sup>123</sup> The main exception is the Arabic

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<sup>122</sup> Wallis to Thomas Smith 16 February 1681, MS Smith 54, f. 29.

<sup>123</sup> Ifrah 1998, 212-213.

alphabet which was rearranged in the seventh or eighth century AD to bring together letters similarly written. This may have made the teaching of reading and writing easier but it necessitated the use of mnemonics to correlate numbers with their respective letters,<sup>124</sup> and perhaps indirectly encouraged the adoption of the Hindu-Arabic system.

§ 3.11 But beside that, (which in great Numbers would be very troublesom) they have another way much more convenient (by Ten Numeral Characters, altering their Values according to the places wherein they stand) as now we have, and which we borrowed from them.

§ 3.12 These Figures, which are wont to be called *Numeri Barbarici*, suppose (for the year) 1676, (in opposition to what are called *Numeri Romani*, MDCLXXVI:) or *Ciphrae Saracenicae*, or *Arabicae*, (because from the *Saracens* and *Arabians* they came to us:) How long they have been in use amongst them, we cannot certainly tell; but that with the *Arabians* and *Persians* they have been much longer in use than with us, I take to be very certain.

This paragraph contains two interesting descriptions, *Barbarici* and *Saracenicae*. The first was used simply to describe what was not Roman (or Greek), and was not necessarily a term of disparagement.<sup>125</sup> ‘Saracen’ was used to describe Arabs or Muslims at the time of the Crusades (1095-1270) so its use as a description of *ciphrae* is a telling indication of another route by which the Arabic numerals may have reached northern Europe, with the crusaders returning from the eastern Mediterranean. In his loose identification of ‘Saracen’ with ‘Arabian’ Wallis missed the important implications of the word.

§ 3.13 Nor do the *Arabians* pretend to have been the first Authors hereof, but do ascribe them to the *Indians*, from whom they borrowed them. Of which I have (in my *Opus Arithmeticum*, chap. 31.) cited an eminent Testimony out of *Al-Sēphadi*, in his Commentary on a Poem of *Tograjī*, where he ascribes to the *Indians*, three things whereof they glory to have been the Inventors; the Book of *Golaila Wa-damna* of a like nature with our *Æsop’s Fables*;) the Game of *Chess*; and the *Numeral Figures*.

Here Wallis introduced the first of the topics he was about to explore in detail, the geographical origin of the numerals. He had, as he said, already touched on this many years before in one of the first books he wrote after becoming Savilian professor, his *Mathesis universalis sive arithmetikum opus integrum*,

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<sup>124</sup> Ifrah 1998, 241-244.

<sup>125</sup> Moyer 1999, 479.

an introduction to arithmetic. There, as part of a discourse on geometrical progression, he had given in both Arabic and Latin the story of the inventor of the game of chess, who sought as his reward the amount of rice to be had by doubling the grains on successive squares of the chessboard. Wallis quoted the story from the commentary of al-Ṣafadī (1297-1363) on the *Lāmiyyat al-'Adjam* of al-Ṭughrā'ī (1061-1121),<sup>126</sup> but it is common in Arab and Persian literature. The importance of the story in Wallis's present context was that, besides the game of chess, it ascribed two other wonders to the Indians: the tale of the *Panchatantra* (the source of the Persian fable *Kalīla wa-dimna*), and the numerals together with place-value.

§ 3.14 And *Maximus Planudes* (in his Book before cited) calls it *Λογιστική 'Ινδική*, and *Ψηφοφορία κατ' 'Ινδους*, *The Indian way of Computation*; and says expressly, *Τὰ δὲ σχήματα καὶ ἀν'τὰ 'Ινδικὰ ἔστιν*; *And these Figures are Indian Figures*.

See § 3.3. In Planudes' treatise both the figures and the methods of calculation are described as Indian. Planudes began by setting out the nine integers 1, 2, 3, . . . 9 (in their eastern Arabic form, identical apart from the '5' with the modern Arabic numerals) and added 0, which he called *tsifra*. He went on to explain the rules for addition, subtraction, multiplication and division. In this he was following the first great text on Indian figures, that of al-Khwārizmī, which no longer survives in Arabic but has been reconstructed from early Latin translations.<sup>127</sup> Al-Khwārizmī's treatise opened with a detailed exposition of the principles of place-value followed by instructions for addition and subtraction, doubling and halving, multiplication and division, all done first for integers then for fractions (common and sexagesimal), and it ended with the extraction of square roots. Later writers followed a similar plan but often treated integers and fractions in separate texts. Planudes, the first known Greek writer on the Indian figures and methods, dealt only with integers and only with the four basic operations of arithmetic.

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<sup>126</sup> Wallis 1657b, Chapter 31. Wallis obtained the translation from Edward Pococke who translated and published the *Lāmiyyat al-'Adjam* with his own detailed commentary in 1661. See also Toomer 1996, 247-248.

<sup>127</sup> Folkerts 1997, 8-25. The treatise is thought to have been called *Kitāb fi 'l-jam 'wa'l-tafrīq* ('Treatise on gathering [addition] and dispersion [subtraction]').

§ 3.15 And a Treatise of *Algorithm* in Verse, of *Johannes de Sacro Bosco*, (or at least subjoined to that of his in Prose, and at least as ancient as it,) begins with these two Verses:

*Haec Algorismus ars praesens dicitur, in qua  
Talibus Indorum fruimur bis quinque Figuris, &c*

Now Wallis moved from oriental to European sources. The early western writers on the new numerals, like Planudes in the east, based their work on al-Khwārizmī's seminal text, and over the course of time his name became corrupted to *algorism* or *algorithm* which became, as here, a general title for such treatises.<sup>128</sup>

One of the earliest thirteenth-century algorisms was the one quoted here, composed in verse and known as the *Carmen de algorismo* (song of algorithm).<sup>129</sup> Wallis was hesitant in ascribing it to Sacrobosco but correct in supposing it was 'at least as ancient'; it was in fact written by a French Franciscan, Alexandre de Ville Dieu (d.1240). Little is known about Ville Dieu<sup>130</sup> but he wrote a treatise on ecclesiastical computation in verse in 1200 so his algorism may be supposed to date from about the same period. It became immensely popular: there are eleven copies in the Bodleian Library, seven in the Digby collection alone, and another in the Savile manuscripts, but it was often copied without author or title so Wallis could be forgiven for failing to identify the writer. The first few lines set out the numerals and explain the principle of place-value:<sup>131</sup>

*Haec Algorismus ars praesens dicitur, in qua  
Talibus Indorum fruimur bis quinque figuris.  
0.9.8.7.6.5.4.3.2.1.  
Primoque significat unum: duo vera secunda  
Tertia significat tria: sic procede sinistra  
Donec ad extremam venias, qua cifra vocatur;  
Quae nil significat; dat significare sequenti.  
Quaelibet illarum si primo limite ponas,  
Simpliciter se significat: si vero secundo,*

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<sup>128</sup> Allard 1987; Folkerts 1997, 6-7. The three surviving twelfth-century redactions of al-Khwārizmī's text are the *Liber ysagogarum alchorismi*, *Liber alchorismi* and *Liber pulveris*.

<sup>129</sup> Halliwell 1839, 73-83; Steele 1922, 72-80.

<sup>130</sup> Ville Dieu was sometimes described as *Dolensis* which suggests that he came from the region close to Mont Dol and Mont St Michel in northern France, probably from the town now known as Villedieu-les-Poêles.

<sup>131</sup> Translated JS.

*Se decies; . .*

This present art is called 'algorismus', in which  
We make use of twice-five Indian figures:

0.9.8.7.6.5.4.3.2.1.

The first signifies one: two the second

The third signifies three: thus proceed left

Until you come to the end, which is called 'cifra';

Which signifies nothing; it gives significance to what is behind it.

If you put any of these in the first place,

It signifies simply itself: if in the second,

Itself tenfold . .

The birthplace of Johannes Sacrobosco (c.1200-1244 or 1256) is uncertain but Wallis took him to be English (see § 2.23).<sup>132</sup> He may have studied in Oxford but spent most of his life in Paris. His *Algorismus* (or *De arte numerandi*) with the opening line *Omnia que a primeva rerum origine* was composed about 1230, a little later than Ville Dieu's *Carmen de algorismo*, and it too dealt with the topics set out by al-Khwārizmī: place-value, addition, subtraction, doubling and halving, multiplication and division, all for integers. To these Sacrobosco added cube roots and an elementary treatment of arithmetic progression. It became the most popular of the medieval algorisms and remained in use as a university text across western Europe for three centuries.<sup>133</sup> As such it set the pattern for all subsequent texts on arithmetic: the same material in much the same order (along with fractions) was covered in the early chapters of Oughtred's *Clavis* last published in 1702, five hundred years after Sacrobosco and almost a thousand after al-Khwārizmī.

Sacrobosco's *Algorismus* is immediately followed by the *Carmen de algorismo* of Ville Dieu in MS Savile 17,<sup>134</sup> a volume well known to Wallis. Other Bodleian Library manuscripts in which the *Algorismus* is followed by all or part of the *Carmen de algorismo* will be discussed in § 4.10.

§ 3.16 'Tis therefore I think not to be doubted, but that we had these

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<sup>132</sup> In Wallis 1693, 6, Wallis argued on etymological grounds that Sacrobosco came from Halifax in Yorkshire. There is no historical evidence to support this suggestion but it has since become commonplace, see Pedersen O. 1985.

<sup>133</sup> There is an early English translation in Steele 1922, 33-51. For a modern critical edition see Pedersen F.S. 1983.

<sup>134</sup> Sacrobosco, *Omnia que a primitiva origine rerum*, ff. 94<sup>v</sup>-104; Ville Dieu, *Hec algorismus ars presens dicitur*, ff. 104-108<sup>v</sup>.



Figures, partly by the way of *Greece* (as those of *Maximus Planudes* a *Grecian*,) and partly by the way of *Spain* (and by this especially, and before the other) from the *Moors* there, who had them from the *Saracens* or *Arabians*, and these either from the *Indians* immediately, or at least they from the *Persians*, and these from the *Indians*.

By 'Greece' Wallis meant the Greek Byzantine empire centred on Constantinople. Wallis's source for the idea of double transmission, through Byzantium and through Spain, was possibly Kircher's *Arithmologia* (see § 3.3). Kircher, like Wallis, had taken some trouble to seek out manuscript evidence, presumably in the Vatican library, and had come to the conclusion that the numerals had arrived from Byzantium through Planudes about 1270, and from Spain through the Alphonsine tables which he dated at 1252 (but which were actually written in 1272).<sup>135</sup> Wallis never gave any further consideration to the eastern route but, as we shall see shortly, argued for a much earlier date than Kircher's for the transmission from Spain. Kircher, for all the wealth of resources in the Vatican, lacked the kind of texts copied and used by working mathematicians in Oxford, and now available to Wallis: it was Wallis's access to Oxford's unique heritage of medieval material which enabled him to carry his argument very much further.

§ 3.17 And to this I find the Learned *Gerard Vossius* to incline (in his Book *De Scientiis Mathematicis*, chap.8.) rather than to that of *Dasypodius*, who thinks them derived from the Letters of the *Greek* Alphabet. And *Vossius* directs to that Rule which will soon determine it, to wit, *If any of the Oriental Nations have Letters or Figures, which do resemble those of ours, those in likelihood are the Authors of them*: Which 'tis sure enough, that those of the *Arabians* do; and that so nearly, that if they had been known to *Dasypodius*, he would not himself have doubted it.

Conrad Dasypodius, writing at the end of the sixteenth century, put forward the idea that the modern numerals were derived from Greek alphabetic numerals and justified it with a table comparing Greek and modern numerals.<sup>136</sup> Vossius discounted his theory, citing the authority of Joseph Scaliger who claimed that the modern numerals did not appear in Greek texts until well after the sack of Constantinople in 1204. Vossius instructed his

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<sup>135</sup> The tables referred to the 'Alphonsine era' which began in 1252 with the coronation of Alphonso X of León and Castile, but were actually compiled 1263-72 and did not reach Paris and Oxford until about 1320. See North 1989a, 327-359.

<sup>136</sup> Dasypodius 1593-96. The relevant section is quoted by Smith D.E. and Karpinski 1911, 33 n. 2. Dasypodius' table is also reproduced in Ifrah 1998, 358.

readers to look instead for similarities with the shapes of oriental letters and figures. Wallis produced a table to demonstrate that the medieval and modern European numerals were related to their Arabic equivalents (he gave the eastern Arabic forms though modern numerals are actually derived from the western Arabic, or *ghubar* numerals used in Spain and north Africa).

§ 3.18 These Figures *Vossius* (in the place cited) calls *Siphers*, (*Barbaras numerorum Notas quas Siphra dicimus, &c.*) and chuseth to write it with *S* rather than *C* or *Z*, as deducing it from the *Hebrew Saphar*, (*numeravit, descripsit*;) and applies it indifferently to all those ten Characters: And so it is commonly used by many others, who call them the *Arabic*, or *Saracen*, *Siphers* or *Ciphers*. And amongst ourselves, to *Cipher* or to *cast Account* are used promiscuously for the skill of using these Figures. And in allusion to that general signification, I suppose, it is, that writing in obscure or unusual Characters is called, writing in *Cipher*; of which *Baptista Porta* hath a Treatise, entituled, '*De Zipheris, sive furtivis literarum notis*'. But the word *Cipher*, however now it comes to be used (synecdochically) of all the ten, yet did originally belong to what we commonly call a *Cipher*, that is, o, (which denoteth *none*;) and the *Arabs* (from whom we have it) call it *Tsiphron*, from *Tsāphera*, (i.e. *Vacuum esse, inane esse*, to be void or empty) which answers to the Hebrew *Tsaphar* (with *Tsade*) *avolavit*; not from *Saphara*, which answers to the Hebrew *Saphar* (with *Samech*) *numeravit*: And so *Maximus Planudes* writes it, and applies it particularly to that note of *Nullity*. For (having recited the nine significant Figures) he adds *Τιθέασι δέ ἑτερόν τι σχῆμα ὁ καλοῦσι τζίφραν, κατ' Ἰνδούς σημαίνον οὐδέν*. *They add*, saith he, (beside those nine) *a figure, which they call Tziphra, which, with the Indians, denotes none*. And again *Ἡ δέ τζίφρα γράφεται οὕτως ο*; i.e. *The Tsipher is thus written, o*: And therefore I think the word is as well written with *C* as with *S*; the Letter *c* (as we in *England* commonly pronounce it before *e* and *i*) having a sound like *s*, but somewhat harder, (as when we write, or some of us, *to advise*, with *s*, but *to give advice*, with *c*;) and therefore fitter to express *ts*.

The gradual change in use of the word *cipher*, from meaning *zero*, to a general *digit* (as in the French *chiffre*), and then *to reckon* (again preserved in the French *chiffrer*), and finally to *secret writing* or *code*, is indeed a fascinating piece of etymology, and one that Wallis could hardly have resisted exploring. Note, though, that although he mentioned *Baptista Porta's De ziferis*,<sup>137</sup> and in 1657 had mentioned several other writers on secret codes,<sup>138</sup> he said nothing about his own lifelong experience as a cryptanalyst.

Wallis's discussion of the use of '*c*' and '*s*' was also typical of him; half of his *Grammatica*<sup>139</sup> had been devoted to the subject of pronunciation, and he

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<sup>137</sup> *Baptista Porta* 1591.

<sup>138</sup> *Wallis* 1657b, Chapter 9.

<sup>139</sup> *Wallis* 1653, 1-67.

was to extend the above discussion even further in the 1693 translation of *A treatise of algebra* (where the English words *to prise*, *to appease*, but *price*, *peace* etc stand out strangely from the Latin text). Not just spelling but meaning was at stake here, for the use of ‘c’ rather than ‘s’ linked the word *cipher* with *tsaphera* (to be empty) rather than *saphara* (to count or reckon).

§ 3.19 To this way of Arithmetic, by these Numeral Figures, they give the peculiar name of *Algorism*, (a word which, I believe, is not to be found any where used more anciently, nor for any other, than this way of Practical Arithmetic,) being an Arabic name, compounded by them of their *Arabic* article *Al*, with the *Greek* *Ἀριθμός*, (in like manner as *Ptolemy’s Almagist*, is by them so called from *Al* and *μεγίστη*) The *Arabic* name of *Algorithm*, or *Algorism*, being of the same age with us, as is the *Arabic* way of Calculation, or Practical Arithmetic. It was anciently called also by another name, *Abacus*; which *Lucas de Burgo* (the first printed Author of this kind) supposeth to have been corruptly spoken for *Arabicus*, as coming to us from the *Arabs*.

Wallis’ derivation of *algorism* has been described as ‘eccentric’,<sup>140</sup> but it is also instructive, for it shows that although Wallis recognised al-Khwārizmī as the inventor of algebra, he had lost sight of him as a writer on arithmetic. He was not alone in this. An early English translator of Sacrobosco’s *Algorismus* struggled to explain the word *algorism* as deriving either from *algos* (art) and *rithmus* (number) (hence the Latin *ars numerandi*), or from *gogos* (introduction) and *rithmus*, or from a mythical Indian king *Algus*, the supposed inventor of the art.<sup>141</sup> All these derivations are to be found in Sacrobosco’s original text though, apart from the last, not so explicitly stated. By comparison Wallis’s suggestion is creditable, and indeed half correct, in that he identified the Arabic origin of the syllable *al*. He was also correct in recognising that the term *algorithm* came into use in Europe at the same time as the Hindu-Arabic numerals and was always specifically associated with Indian methods of calculation.

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<sup>140</sup> Molland 1994, 217.

<sup>141</sup> Steele 1922, 33. Pages 3-32 of Steele 1922 contain an early English commentary on Ville Dieu’s *Carmen de algorismo* in which the anonymous writer expounds similar ideas about the origin of the word *algorism*: ‘Ther was a kyng of Inde, the quich heyth Algor, and he made this craft. And after his name he called hit algorym; or els another cause is quy it is called Algorym, for the latyn word of hit Algorismus comes of Algos, grece, quid est ars, latine, craft on Englis, and rides, quid est numerus, latine, or nombur on Englys, . . . quasi ars numerandi.’

*Abacus* was a Latin word, derived from Greek *ἀβαξ* for a counting board, and not a corruption of *Arabicus*. In later (sixteenth-century) European literature the *algorists* were commonly set against the *abacists* as representatives of the new methods versus the old.<sup>142</sup> *Abacus* texts, however, belonged to a different tradition which had nothing to do with the old abacus methods; rather, they arose in Italy in the thirteenth century from the *Liber abbaci* of Leonardo of Pisa (Fibonacci). This and succeeding texts of the same kind taught written methods of computation using Hindu-Arabic numerals, unlike the earliest algorithms which taught ‘dust-board’ methods in which numerals were erased as calculations were performed.<sup>143</sup> Hence *abbacus* arithmetic was rather closer than *algorithm* to modern computation.

§ Chapter 4. How ancient the use of Numeral Figures hath been in these Parts of the World

§ 4.1 As to the Time when these *Numeral Figures* began first to be in use amongst us; *Vossius* tells us (in the place cited), that they have not been in use above 350 years; at least, not 400 years at the utmost. *Non nisi anni sunt CCCL, saltem infra Quadringentes, quod eas Sifras accepimus.* which Book being written about the year 1650, (as appears by the date of the Epistle prefixed;) it is as much as to say, they were not in use till the year 1300; or, at the farthest, not before 1250.

The sentence in italics is cited directly from *De Scientiis*.<sup>144</sup> Wallis might also have quoted Kircher who also argued that the numerals had arrived during the period 1250-1300 (see § 3.16). Wallis suspected a much earlier date and the problem prompted him to new research, and was the theme of this, the fourth, of his opening chapters.

§ 4.2 But I take them to be somewhat more ancient than so, perhaps not in common use, but at least in Astronomical Tables: For I suppose they were first of all admitted in the Astronomical Tables, which we transcribed from the *Moors* or *Arabs*; and afterwards, by degrees, came into common use; till at length they began to be generally used in all Arithmetical Operations, as being much more convenient for that purpose than other ways of designing Numbers.

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<sup>142</sup> There is a well known wood-block engraving of Lady Arithmetic presiding over a smiling algorist and a gloomy abacist in Reisch 1503 and an illustration of the *Quarrel of the Abacists and the Algorists* in Recorde 1551.

<sup>143</sup> Van Egmond 1994, 200-209.

<sup>144</sup> Vossius 1650, 34.

Wallis, like Kircher, recognised that the numerals made an early appearance in astronomical tables; unlike Kircher, he recognised that the Alphonsine tables were not the first to spread beyond Spain (see § 4.8).

§ 4.3 I know that in the Editions which we now have of *Boëtius*, *Bede*, and other ancient Authors, these [Arabic] figures are now frequently used: but I do not believe they were found in the ancient Manuscript Copies, from whence these printed Copies were taken; but in those, all their Numbers were expressed by the *Latin* Numeral Letters, (and in divers ancient Manuscripts I have so seen it:) And therefore I do not bring those as an argument of their Antiquity, nor do I believe they were in use (in these western Parts) when those authors were first written.

Numerals were often changed and updated in the course of copying, not only from manuscript to print, but from manuscript to manuscript (the same thing could happen with diagrams<sup>145</sup>). MS Savile 20 and MS Selden Supra 25 both contain copies of Boethius' *Arithmetica* which use Roman figures but in both copies Arabic numerals have been added alongside or in the margins. Wallis would certainly have known the first of these and probably the second also.

In the twentieth century Smith and Karpinski discussed at some length the question of whether Boethius could have known the Indian numerals by way of the trade routes from the far east,<sup>146</sup> and cited this paragraph from Wallis as part of their rejection of such a hypothesis.

§ 4.4 But that they are somewhat more ancient than *Vossius* mentions, I judge for these Reasons:

§ 4.5 First, I find in our *Savilian* Library divers ancient Manuscripts in which these figures do occur; (in some, perpetually; in others, very frequently.) Amongst which, there be two compleat Volumes of Astronomical Tables, for all the Celestial Motions, and two Calendars for the Ecclesiastical Account; all of them fairly written in excellent good Vellum, with great accurateness and cost; which I judge from divers circumstances there appearing, to have been written not long after the year 1200, at least before 1250: Beside many other Astronomical Treatises, (translated divers of them out of *Arabic*) which appear to be much about the same age.

All the works Wallis described here: tables, calendars and other treatises are found together in a single volume now known as MS Savile 21. This was a volume Wallis knew well: he made extensive annotation on the blank flyleaf at the front of the volume and brief notes on the corresponding pages of the

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<sup>145</sup> Netz 1999.

<sup>146</sup> Smith D.E. and Karpinski 1911, Chapter 5.

texts themselves, and his annotations are all concerned with dating. The modern Bodleian catalogue gives the date of copying as ‘thirteenth century’, a conclusion less precise than Wallis’s.

§ 4.6 But when I say, *not long after* 1200, I do not know, but some of them may have been written a good while before that time, especially those two Volumes of Astronomical Tables: For they are (one or both of them) the Tables of *Arzachel*, a *Moor* in *Spain*, whom *Vossius* says to have been eminent in *Spain*, about the year 1080; (but says also, that some others judge him to have been more ancient.) His Tables are accommodated to the Meridian of *Toledo*; and were written, I presume, in *Arabic*, (because, by a *Moor*, and accommodated to the *Arabian* year,) but translated into *Latin*, and so brought into *England*, by some of ours, who went on purpose into *Spain* to learn the *Arabic* Language, and to be acquainted with this kind of Learning; which was then to be learned no where but of the *Moors*, and out of *Arabic* Authors: Which Authors were not to be understood, nor the Tables translated into *Latin*, without knowledge of the *Arabic* Figures, (or as they be there called, *Indian* figures) retained (with some little alteration) in the *Latin* Translations, which we have.

The Toledan tables were written not by *Arzachel*, as Wallis supposed, but were compiled between 1062 and 1078 from the earlier tables of al-Khwārizmī (c.830), al-Battānī (c.888) and Thābit ibn Qurra (c.870). *Arzachel* (al-Zarqālī of Cordoba, d. 1100) wrote the associated *Canones tabularum*, or explanations, which are found twice in MS Savile 21 (at ff. 27-41<sup>v</sup> and ff. 63-103). Before the Alphonsine tables were compiled in 1272 the Toledan tables were used throughout Europe and adapted for other centres: Marseilles (c.1140) and Oxford (1150).<sup>147</sup> Those in MS Savile 21, ff. 63-103 were translated by Robert of Chester (see § 2.17), in this case described as Robertus Cestrensis.

§ 4.7 Finding therefore, that divers of our own Nation (to say nothing of others) did on this account travel into *Spain*; as *Adelardus*, about the year 1130; and *Retinensis*, about 1140; *Shelley*, about 1145; *Morley*, about 1180; it must needs be, that these Figures were in use with us, a good while before the year 1250: And, that they came into use, at the same time with this sort of *Arabic* Learning. and those who translated the *Arabic* Authors into *Latin*, (amongst whom was *Johannes Hispanicus* or *Hispalensis*, whom *Vossius* placeth about the year 1140) must needs be thought to have made use of these figures, which we find used in the oldest Manuscripts (that I have yet seen) of the *Latin* Translations of those *Arabic* Authors.

All the authors listed here have already been discussed (§ 2.15-§ 2.20) except John of Spain (or John of Seville, fl.1133-1142), the only non-English

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<sup>147</sup> North 1986, 114-117.

translator of Arabic texts ever mentioned by Wallis. He was one of several Jewish scholars active in Spain, and a prolific translator of astronomy and astrology from Arabic to Latin. He would have been known to Wallis from his translation of the treatise on the astrolabe written by Messahalah (Māshā'allāh, fl.762-c.815), to be found in MS Savile 21 (ff. 104-115) and probably the reason for Wallis adding his name here.

§ 4.8 And that not only the first Copies of these Translations, but even these particular Books, are more ancient than the *Alphonsine* Tables, (first published, as *Vossius* tells us, in the year 1270; others say, in the year 1252;) because when these were once made, those of *Arzachel* grew out of date: And whoever would be at the cost and care to have Astronomical Tables so fairly written, would chuse to have those which were latest, and reputed most accurate.

Wallis argued correctly that any copy of the Toledan tables must have been made before the Alphonsine tables superseded all others. His argument is interesting in that he here saw the tables from the point of view of those who paid for them, a useful reminder that mathematics required its patrons as well as its practitioners. This appraisal of the situation from an economic as well as intellectual perspective is another of the modern aspects of Wallis's historiography.

§ 4.9 'Tis certain also, that *Johannes de Sacro Bosco*, whom *Vossius* places about the year 1232, (and who died in the year 1256) was not only acquainted with them, but hath left one or two Treatises *De Algorismo*; shewing the use of these Figures in all parts of Arithmetic, and doth appropriate to them the name of *Algorismus*. Two copies we have of it in Manuscript; one in the *Bodleian* Library, the other in the *Savilian*: which Art he divides into nine parts; *Numeration, Addition, Subtraction, Mediation, Duplation, Multiplication, Division, Progression, and Extractions of Roots, Square and Cubic*; Which are there performed much in the same manner as they are at this day.

The year of Sacrobosco's death comes from his tombstone in the Convent of St Mathurin in Paris but the last three words of the date in the Latin inscription, '*M christi bis C. quarto deno quater*' are ambiguous and may be read as 'four tens plus four' (giving 1244) or 'four fourteens' (giving 1256). Vossius chose the second interpretation, in which Wallis followed him. Modern scholars remain uncertain and have suggested other possibilities.<sup>148</sup>

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<sup>148</sup> Pedersen O. 1985, 186-192.

There is one copy of Sacrobosco's *Algorismus* in the *Savilian* Library, in MS Savile 17 (ff. 94<sup>v</sup>-104). The identity of the manuscript Wallis described as being in the *Bodleian* Library will be discussed under § 4.10.

As has already been described at § 3.15, Sacrobosco's *Algorismus* set the pattern for all later European arithmetic texts, and Wallis would have been thoroughly familiar with the ordering of the material (apart from *mediation* and *duplation* which fell out of use as separate headings); indeed, his own *Mathesis universalis* of 1657 dealt, from Chapter 10 onwards, with the same pedagogical material, with very much greater sophistication and detail but in the same order.<sup>149</sup>

§ 4.10 And to this Treatise in Prose, there is (in both Copies) subjoynd another in Verse (as was the fashion of those times) to the same purpose: which therefore I judge to be his also, though his Name be not put to it; and if not, 'tis at least as ancient: for his in Prose cites this in Verse.

The juxtaposition of prose and verse enables us to identify the copy that Wallis said was in the *Bodleian* Library since Sacrobosco's *Algorismus* is followed by Ville Dieu's *Carmen de algorismo* in MS Bodl. 177 and MS Digby 190.<sup>150</sup> In MS Bodl. 177 the two texts are interwoven with each chapter of the *Algorismus* followed by the corresponding verse of the *Carmen de algorismo*, though only up to the fifth verse where the writing breaks off and a blank page still awaits completion;<sup>151</sup> in MS Digby 190 the *Algorismus* is followed by the first two verses of the *Carmen de algorismo* but written in prose form.<sup>152</sup> The manuscript which best fits Wallis's description is therefore MS Bodl. 177. Though Wallis was mistaken in ascribing the *Carmen de algorismo* to Sacrobosco, he was correct in his relative dating of the two pieces.

§ 4.11 Now he dying (of a good age) in the year 1256, (and being well versed in these Studies) we may well think, this Treatise might be written divers years before 1250. And though, of some other Books, where we find such Figures used, it may be thought they might possibly be used in later Transcripts, though the originals had been written with the *Roman*

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<sup>149</sup> Wallis 1657b, Chapters 10-34.

<sup>150</sup> The *Algorismus* also appears with the *Carmen de algorismo* in MS Add.C.93 but this was not acquired by the Bodleian Library until the 19<sup>th</sup> century.

<sup>151</sup> MS Bodl. 177, ff. 45-45<sup>v</sup>.

<sup>152</sup> MS Digby 190, *Algorismus*, ff. 169<sup>v</sup>-175; *Carmen de algorismo* (opening only), f. 175.



Numbers, (as was said before of *Boëtius*, *Bede*, and others;) yet, in these, it must needs be, that the Figures are as ancient as the original, because the scope of the Book is to teach the use of them.

§ 4.12 And in whatever Authors we meet with the name of *Algorism*; so old, at least, we may conclude the use of these Figures to have been.

Wallis here repeated the important historiographical point he had already made in § 3.19, that the very purpose of the *Algorisms* was to teach the methods associated with the new numerals so that the title alone can always be taken as an indication of their use.

§ 4.13 In another Book of the same author, *Johannes de Sacro Bosco*, which is *De Computo Ecclesiastico*, (of which we have an ancient Manuscript Copy, wherein these Figures are also used,) he says expressly (which shews the time wherein it was first written) *Ab incarnatione Domini elapsi sunt 1235 anni*; and therefore more ancient than either 1300 or 1250.

Sacrobosco's *De computo ecclesiastico* (beginning *Computus est sciencia considerans tempora*) noted the increasing error in the Julian calendar. It is to be found in MS Savile 17 (ff. 141-174<sup>v</sup>), a volume in which, as in MS Savile 21, Wallis made several annotations concerned with the dating of the texts. In particular he carefully transcribed onto the flyleaf the words '*Ab incarnatione domini elapsi sunt 1235*' which appear in Sacrobosco's text.

§ 4.14 I find also by a Treatise of *Robert Grosseteste* (Bishop of *Lincoln*), *De Computo Ecclesiastico*, with a Calender annexed (fairly written in an ancient Manuscript in Vellum) that they were used by him also, who flourished about the same time. He was made Bishop of *Lincoln* in the year 1235, and died in the year 1253.

Before becoming bishop of Lincoln, Grosseteste was *Magister scholarum* in Oxford (1214-1231) and *Lector* to the Oxford Franciscans (1232-1235). During his Oxford period he wrote a number of scientific treatises including his *Computus*, first written c.1210, corrected 1215-1219 and revised again in 1244. In it he noted the discrepancies between lunar and solar time and, like Sacrobosco, suggested appropriate reforms.<sup>153</sup>

Grosseteste's *Computus* in MS Savile 21 (ff. 127-142<sup>v</sup>), beginning *Computus est sciencia numeracionis et divisionis*) is the first revised version from 1215-19 and has been copied using Arabic numerals. It is not known when the copy was made but there is other evidence, to be discussed in § 4.18, that Grosseteste was indeed using the new numerals by 1215.

§ 4.15 And *Roger Bacon*, whom *Vossius* placeth about the year 1255, (a person so well skilled, and so well acquainted with *Arabic Learning*, and so intimate with the persons last mentioned, as we find him to have been) cannot be thought to have been ignorant herein.

Roger Bacon (c.1214-1292), a great admirer of Grosseteste, argued for the usefulness of mathematics in every part of intellectual activity,<sup>154</sup> but his own contribution to the subject, his *Communia mathematica* was of little consequence. He did, however, have a good understanding of the shortcomings of the Julian calendar and suggested some practical corrections.<sup>155</sup> His learning became almost legendary. Vossius later wrote: ‘He was a man both learned and subtil unto a miracle, and did such wonderfull things by the help of mathematicks that by such as were envious and ignorant he was accused of diabolicall magick’, an adulatory opinion which Wallis shared. Wallis was certain that a man so learned must have known the new numerals, but his paragraph here is notable for its lack of any evidence that Bacon used them during the period of interest to Wallis, before 1250. In fact most of Bacon’s scientific writings date from after 1266, too late for Wallis’s purposes.

§ 4.16 And *Alexander de Villa-Dei, Dolensis*, whom *Vossius* says to have lived about the year 1240, and to have written of Arithmetic, and ecclesiastical Computation, did, I presume, therein make use of these Figures. For though I do not remember that I have seen these Books, (at least not under that name;) yet these being then in use, and so convenient for that purpose, it is not likely that he would wave them, and make use of Numeral Letters, which are much more troublesom and inconvenient.

This was Wallis’s first mention by name of Alexandre Ville Dieu whom he had found (described as *Dolensis*) in the pages of *De scientiis*.<sup>156</sup> Vossius noted Ville Dieu as a writer of arithmetic, but not of verse, so that Wallis never recognised him as the writer of the *Carmen de algorismo* (which he had now cited twice). Wallis thought that he had never seen Ville Dieu’s ecclesiastical computation either: he was wrong here too, as will be shown in § 4.19.

§ 4.17 We have also, in Manuscript, another Treatise of *Algorism*, of *Jordanus*, (whom *Vossius* placeth about the year 1200, and Contemporary

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<sup>153</sup> North 1989b, 44-46; North 1992b, 131-132.

<sup>154</sup> Bacon 1928, 117-127; Grant 1974, 90-94.

<sup>155</sup> North 1989b, 46-48; North 1992b, 132.

<sup>156</sup> Vossius 1650, 40.

with that *Campanus*, who wrote *De Computo Ecclesiastico*;) entitled, *Algorismus Jordani, tam in Integris quam in Fractionibus, demonstratus*; in which, the use of these Figures, and the way of numbering by them, is with great accuracy described and demonstrated. Which *Algorismus* of his is very different from his *Arithmetica*, published and illustrated by *Faber Stapulensis*; yet so, as it may very well be judged, by his manner of demonstration, to be a work of the same man. And the Manuscript it self, as appears by the hand, and by the shape of the Figures, is very ancient.

Vossius said that Campanus (of Novara, d. 1296) considered Jordanus (fl. 1220) famous for his work on the astrolabe, and that Jordanus in his treatise on weights mentioned Campanus, and hence, argued Vossius, they must have been contemporaries.<sup>157</sup> He was mistaken in this since Campanus wrote his major works around 1260, some forty years later than Jordanus. However, Jordanus was so renowned for his treatise on weights that many later commentaries and treatises on the subject were wrongly ascribed to him. Wallis, as usual where he had no evidence to the contrary, followed Vossius.

The identity of Jordanus remains a subject of controversy and uncertainty.<sup>158</sup> A number of mathematical treatises are ascribed to him, of which that on weights, his *Elementa Jordani super demonstrationem ponderum* is perhaps the most important.<sup>159</sup> The algorism ascribed to him is usually known as *Demonstratio Jordani de algorismo*, with an additional section on fractions, the *Demonstratio minutiis*; both are copied in MS Savile 21 (ff. 143-150) with the heading noted by Wallis. Like Sacrobosco, Jordanus covered the operations of addition, subtraction, doubling, halving, multiplication, division and extraction of roots, but his treatment was more formal and without examples. Jordanus' work was firmly rooted in the Euclidean tradition of stating propositions and demonstrations, and he seems to have eschewed eastern influences for although he presented the new Arabic numerals in his *Demonstratio*, he used them very little. All the earliest extant copies of another of his works, his *De numeris datis* use Roman numerals, which are fully replaced by Arabic only in much later copies.<sup>160</sup>

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<sup>157</sup> Vossius 1650, 178.

<sup>158</sup> Clagett 1959, 72-73; Klein O. 1964.

<sup>159</sup> Clagett 1959, 69-159.

<sup>160</sup> Hughes 1981, 22-38.

The *Arithmetica* of Jordanus, also mentioned here by Wallis, was also written as a series of formal definitions and propositions,<sup>161</sup> and Wallis, in an interesting example of verifying authorship from mathematical style, noted the similarity ‘in manner of demonstration’ between this work and the *Demonstratio Jordani de algorismo*. The *Arithmetica* became a standard source of theoretical arithmetic; Jacques Lefèvre d’Etaples (Jacob Faber Stapulensis) published the propositions with his own demonstrations in 1496, but only in recent years has it been printed in full.<sup>162</sup>

Note Wallis’s introduction of yet another historical method here: dating by handwriting. Though it did not enable him to establish a precise date in this case, he did recognise the useful link between period and style.

§ 4.18 And in the same Manuscript Book, wherein that of *Jordanus*, and some other small pieces are written, I find at the end of it two Celestial Schemes, relating to the year 1216; the one of them is called *Figura Anni*, representing the Position of the Heavens on *March 22.1216*; the other, *Figura Conjunctionis Saturni & Martis*, shewing the Position of the Heavens at the time of that Conjunction which happened the same year, *October 4. 1216*. They are both of them described by these Numeral Figures; and, in likelihood, were calculated about that time, in order to some Astrological Predictions to made thereupon. And it so happens, that this last page of that Piece, proves to be the latter leaf of that same piece of Parchment, which begins that Book of *Algorismus Demonstratus*, and therefore later written than it.

The ‘manuscript book’ that Wallis described here is actually written in two sections of eight pages each, all from the same parchment. Note his careful observation of the construction of the manuscript as well as its written content. The book is now incorporated into MS Savile 21 (ff. 143-160<sup>v</sup>). It begins with the *Demonstratio Jordani de algorismo*, continues with copies of astronomical treatises of Thābit ibn Qurra, and ends with horoscope diagrams, the ‘Celestial Schemes’, for use in 1216. The untrained modern reader would have difficulty in finding, let alone understanding, the sentences that date the diagrams, but Wallis transcribed them in full into the flyleaf of MS Savile 21. The importance of the diagrams, as Wallis saw, is that they date the entire section as having been copied before 1216. Modern scholars have identified the

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<sup>161</sup> Jordanus 1974.

<sup>162</sup> Lefèvre 1496; Busard 1991.

handwriting with near certainty as that of Robert Grosseteste.<sup>163</sup> This section is evidence, therefore, that Grosseteste was familiar with the new numerals by 1216 (see § 4.14).<sup>164</sup>

§ 4.19 I find them also used in an ancient Treatise of Ecclesiastical Computation, in Verse, called *Massa Computi*, of which I have seen diverse Copies in Manuscript, (and I think it is also printed:) The Verses of which, I find frequently cited in later Computists. And (though I do not know the Author) that we may not doubt the age, the Work it self declares it; for, where he teacheth how to find the Solstices and Equinoctials at that age, he tells us, that in 120 years they go back one day; and that at the birth of Christ, the Winter Solstice was on *Christmas* day; but falling backwards one day in 120 years, and ten times 120 years (that is, 1200) being then past, it was now come back from the 25<sup>th</sup> to the 15<sup>th</sup> of *December*. His words are these:

*Solstitium quinis horam praecedit in annis,  
Cumque diem faciant viginti quatuor horae,  
Annis viginti centumque dies datur una.  
Solstitium legimus Christo nascente fuisse.  
Centum viginti decies jam praeteriere  
Anni. Sic denis praecedit meta diebus.*

This ecclesiastical computation in verse, the *De computo ecclesiastico* was, like the *Carmen de algorismo*, the work of Ville Dieu.<sup>165</sup> Here Wallis said he did not know the author, whereas in § 4.16 he had named the author but said he had never seen his work. Ville Dieu's *De computo ecclesiastico* like his *Carmen de algorismo* exists in numerous copies: Wallis certainly knew it in MSS Savile 17 (ff. 175-184<sup>v</sup>) and Savile 21 (ff. 161-175). Dating mathematical texts, as here, from their internal content is still a useful historiographical method.<sup>166</sup>

§ 4.20 But though we may hence gather the age of this Work to have been about the year 1200; yet I confess it doth not, from here alone, follow certainly, that these Figures were then in use, however we now find them in some of those Copies which we have; for it's possible, that in the first Original, the numbers here (as well as in *Bede's* Books, *De Computo*) might be designed by Numeral Letters: And so in one Copy I find it to be. But in others, the Numbers are designed by the Numeral Figures; and (these appearing otherwise to have been in use at that time) we may as well think, they were so used in this: Yet so, as that the Numeral Letters were in use

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<sup>163</sup> See Thomson 1940, 22-36, which includes a facsimile of a fragment from MS Savile 21; Hunt 1955, 133-134; Clanchy 1979, 128; Southern 1986, 107.

<sup>164</sup> Williams 1998, suggests that Grosseteste made the copy in southern France c.1213.

<sup>165</sup> Steele 1909-40, VI, 268-283.

<sup>166</sup> Van Maanen 1993.

also, as even to this day they are.

In MS Savile 17 Ville Dieu's *De computo ecclesiastico* has been copied twice, first with Roman numerals then with Arabic. As a calendrical work, unlike an algorism, it could just as well be written either way, and was perhaps originally composed using Roman numerals and updated to the Arabic system later. This presents the historian with the problems Wallis had already warned about at § 4.3 in relation to the work of Bede and Boethius.

§ 4.21 Beside what hath been already said, we have also a Treatise of astronomical tables of *Robertus Cestrensis*, (according to the Doctrine of *Albategnius Aracensis*) by him accommodated to the Meridian of *London*, and adjusted to the beginning of the year 1150, beginning the year at the first of *March* (that the Intercalations in *February* might cause no disturbance in numbering the days); having before (as he there tells us) compiled a like Treatise adjusted to the Meridian of *Toledo*, (according to *Abenezra*, or *Abenarza*, whom in that he follows) beginning at *Jan. 1. 1149.* (as he doth his from *March.1.1150.*) which argues, that he lived about that time, and that these Figures were then in use; For the *Latin* Numeral Letters are altogether improper for Astronomical Tables, nor do I believe that any such were ever written by those Letters: Though some indeed have been written in the *Greek* Numeral Letters (as those of *Ptolemy*), which, though less convenient than the *Indian* Figures, are yet much fitter for that purpose than the *Latin* Letters.

Robert of Chester's translation of the canons of Arzachel in MS Savile 21 has already been noted at § 4.6. *Albategnius Aracensis*, written in the manuscript as 'Albatem Haracensis' was al-Battānī (c.888 of Ḥarrān in Mesopotamia); *Abenezra* was Rabbi Abraham ben Meir ibn Ezra (1090-1164 of Toledo), who translated from Arabic to Hebrew and did much to disseminate Arabic scientific learning. Wallis was correct in supposing that no astronomical tables were ever compiled in Roman numerals.

Robert's translation of the *Canons* was one of the sections of MS Savile 21 annotated by Wallis with particular reference to its date: 1150 appears in Arabic in the tables themselves but is written as *m.c.l.* in the prologue which was presumably added at the time of adaptation. Robert's name appeared as Robertus Cestrensis and Wallis never made the identification with Robertus Retinensis, the translator of the Koran (see § 2.17). He did, however, go to some trouble to identify Robert of Chester as the next two paragraphs show.

§ 4.22 I am not ignorant that *Balaeus*, amongst his Writers of an *uncertain time*, mentions one *Robertus de Cestria*; and says, that *Leland* thinks he might have lived about the time of *Richard* the Second; that is, about the year 1380. But either that must be another of that name, or else *Leland*

mistakes his age: For it is not likely if he lived about 1380, he would have adjusted his Tables to a time so long past, (those for *Toledo*, to the beginning of the year 1149; and those for *London*, to the end of it;) but rather (as in such cases is usual) to his own time, (as *Prophatius Judaeus* doth his, to the year 1300, when himself lived.) Nor doth he therein take notice of the Alphonsine Tables, and divers others which were more ancient than the year 1380; but only of *Albategnius* (whom *Vossius* placeth about the year 888), and *Aben-Ezra* (whom *Vossius* placeth about the year 1145:) Nor do I find him to mention any more late [sic] than that time.

Vossius drew heavily on the work of Bale and Leland but this is the only hint that Wallis himself turned to Bale: perhaps the puzzle of Robert of Chester's identity led him to check the source directly. Bale made entries for *Robert Ketenensis* in both his *Summarium* of 1548 and his *Catalogus* of 1557-59, describing his travels, his friendship with Hermann of Carinthia and his translation of the *Koran* for Peter of Cluny.<sup>167</sup> As there is no mention of mathematics in either case there was no reason for Vossius to take up the accounts. Wallis missed them altogether: he would not have thought of searching the index for *Ketenensis*, a name he never used. He did, however, find an entry in the 1557-59 *Catalogus* (in Part II, which is indexed and paginated separately from Part I) for *Robertus Chestre, vel de Cestria* whom Bale (explicitly following Leland) placed in the reign of Richard II.<sup>168</sup> If Wallis had turned from Leland and Bale to Vossius' third English source, John Pits, he would have seen him too struggling with the problem of Robert's identity. In the *Relationum historicarum*, after a long list of authors in chronological order, Pits added an *Appendix* of 378 further writers for whom he was uncertain of the dates. Among them were *Robertus Cestrensis* immediately followed by *Robertus Cestria* who was said to have died in 1390.<sup>169</sup> Pits clearly knew little of either and seems to have confused the two. Wallis, however, realised that Robert of Cestria was far too late to be a copyist of twelfth-century tables.

§ 4.23 I should rather have taken it for *Robertus Cestrensis*, made Bishop of *Chester* by *William* the Conqueror, in the year 1085 (according to *Simeon Dunelmensis*), or 1087 (according to *Rudolphus de Diceto*). Or 1088 (according to *Godwin*); whom *Dunelmensis* reckons also by the name of *Robertus Cestrensis*, as present amongst others at a Council of Bishops

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<sup>167</sup> Bale 1548, 85<sup>v</sup>; Bale 1557-59, part I, 191.

<sup>168</sup> Bale 1557-59, part II, 52.

<sup>169</sup> Pits 1619, 900.

under *Anselm*, in the year 1102. But *Godwin* calls him *Robert de Limesey*, and says, he died in the year 1116, which is too soon for our purpose. Nor do I meet with any thing concerning his skill in Mathematics. And it is not likely that he would begin his Tables from the year 1149, or 1150, a time then to come; and therefore it must be some other of that name, somewhat later, who lived about the year 1150.

Wallis's persistence in trying to identify Robert of Chester is shown by the fact that he consulted three different historians: Simeon of Durham, Ralph de Diceto and Francis Godwin. Simeon, a precentor of Durham, and Ralph de Diceto, dean of St Paul's, were twelfth-century chroniclers of English history, and the *Historia de gestis regum anglorum* (ending at 1129) of Simeon is to be found with the *Abbreviationes chronicorum* (ending at 1201) of Diceto in Roger Twysden's *Historiae anglicanae scriptores decem*.<sup>170</sup> Simeon and Diceto were the first and fifth of the ten medieval writers published for the first time by Twysden in this weighty but apparently very popular tome.<sup>171</sup> It is still to be found in Duke Humfrey's Library where Wallis probably consulted it,<sup>172</sup> and is a good example of the new accessibility of medieval material to seventeenth-century historians. Frances Godwin (1562-1633) was bishop of Llandaff and then Hereford, and author of *A catalogue of the bishops of England*.<sup>173</sup> The entry for Robert, called Robert Limesey, is found under the bishops of Coventry and Lichfield but indicates that he was ordained at Chester in 1088. There is no hint in any of these accounts, however, that bishop Robert travelled to Spain.

§ 4.24 And I doubt not, but if we make search in our old Manuscripts about that age, we may find the use of them in the 12<sup>th</sup> and 13<sup>th</sup> Century, if not before.

§ 4.25 To this, I add what I have lately seen. At the Parish of *Helmden* in *Northamptonshire*, (in the house of Mr. *William Richards*, now Minister there) on an ancient wooden Mantle-tree to the Chimney in his Parlour,

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<sup>170</sup> Twysden 1652.

<sup>171</sup> Thomas Hearne, Bodleian librarian in 1712, wrote of this book that 'Even puritans displayed something like patriotic ardour in purchasing copies of this work as soon as it appeared'.

<sup>172</sup> Duke Humfrey's Library, built in 1488, is the oldest part of the Bodleian Library. Twysden is shelved, as probably it has been since it was acquired, in the Selden End completed in 1636.

<sup>173</sup> Godwin 1601.



(perfectly black with age and smoke, but firm and hard,) there is carved work (well enough for that age) from the one end to the other; and about the middle of it this date, (in old Carving, not yet defaced,) *A<sup>o</sup> DO<sup>i</sup> M<sup>o</sup> 133*. But both the Letters and Figures of an antic shape, agreeing with that age.

§ 4.26 So that I do not doubt, but that they have been in use amongst us in *England*, at least as long ago as the year 1133; not only in Astronomical Tables, (though first introduced on that occasion). But elsewhere also: Which is near 150 years before the time that *Vossius* mentions.

The village of Helmdon lies about thirty miles north of Oxford and three miles from Marston St Lawrence, the home of Wallis's daughter, Anne, after her marriage to John Blencow in 1675. William Richards was the incumbent of Helmdon from 1675 to 1705. In addition to his careful verbal description of both the physical condition and style of the lettering Wallis arranged to have a drawing made, which he reproduced as a fold out page in *A treatise of algebra* and also published in the *Philosophical transactions*.<sup>174</sup>

Wallis's claim for such an early date triggered a controversy that went on well into nineteenth century. In 1800 Ralph Churton, rector in the neighboring parish of Middleton Cheney, wrote to the *Gentleman's magazine*.<sup>175</sup>

Few of your Antiquarian readers need to be informed how much the inscription on the mantle-tree in the parsonage at Helmdon, in Northamptonshire, has puzzled the learned and curious in such matters ever since the celebrated Dr Wallis gave an account of it in the *Philosophical transactions* above a century ago.

Churton provided a full size tracing of the inscriptions (considerably more accurate than the drawing published by Wallis) and concluded:

As to the decyphering . . . having carefully examined the inscription four severall times [sic], and copied on thin paper with black lead all the material parts twice as often, I am satisfied, upon the whole, that Dr Wallis gave the true reading, namely, 'an<sup>o</sup>. Do<sup>i</sup>. M<sup>o</sup>. 133'.

Thirty years later, however, George Baker published the first volume of his painstakingly researched *History and antiquities of the county of Northampton* and after carefully weighing the evidence came to a different conclusion.<sup>176</sup>

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<sup>174</sup> *Treatise of algebra*, 12-13; Wallis 1683.

<sup>175</sup> Churton 1800, 1232.

<sup>176</sup> Baker 1822-41, I, 631; Gough 1867.

Much disputation and ingenious conjecture have been exercised in decyphering this famous date, and 1133, 1233, 1533, and 1555 have been severally suggested. Some writers have referred the initials W.R. following the date to William Renalde or Reynolde, the rector from 1523 to 1560, and the general style of the mantle-piece, its very depressed arch, and the elongated leaves in the spandrels, certainly correspond with that period, and corroborate the supposition; whilst, on the other hand, it must be admitted that the form of the M and the connecting figures strongly favour the interpretation given by Dr Wallis. From a careful examination of the original I am inclined to attribute this singular curiosity to the rector [Reynolde], though it must be confessed his motive for introducing a fictitious date in rude or arbitrary characters, unless to puzzle future antiquaries seems inexplicable.

Later in the nineteenth century the vicarage was modernised and the mantelpiece, after standing in the porch exposed to weather, was taken into the church for safe-keeping. There it can still be seen, but uncertainty as to its date persists. Architectural experts argue that the carved rosettes are typical of a much later period. A recent opinion (March 2000) states:<sup>177</sup>

This is a very nice bressumer but it is certainly not twelfth century!

The carving is of provincial quality only, and the rosettes which are the only stylistically datable feature, look to be 1400-50. It is impossible to be more precise than that.

A second expert (June 2000), however, considers that the dragon 'could easily be twelfth century work' and admits the possibility that the piece was originally a twelfth century lintel converted to a mantelpiece around 1500.<sup>178</sup>

None of the claims for a later date, as Baker pointed out, offers any credible alternative reading of the carved date, or takes into account the early form of the 3s. It cannot be completely ruled out that Wallis's reading was correct and that the beam was first carved, perhaps as a roof beam or lintel, in 1133, using numerals learned in the course of the early Crusades. The rosettes,

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<sup>177</sup> Charles Tracy FSA, personal communication to the Rector of Helmdon, received 13 March 2000.

<sup>178</sup> Ron Baxter, personal communication to the author, 29 June 2000.

more deeply carved than the numerals, could have been added later: the beam is attractive (the ‘provincial quality’ gives it a pleasing and homely feel) and it is easy to understand why successive generations might have put it to new use rather than see it destroyed. It is also possible that the initials read by Wallis and Baker as ‘W.R.’ could be ‘W.K.’; it would not be the first time that Wallis mistook a medieval looped ‘K’ for an ‘R’ (see § 2.17), though the alternative reading sheds little more light on the date, which for the moment must remain uncertain.

In the ‘Additions and Emendations’ added before *A treatise of algebra* went to press Wallis gave details of another inscription, from the gate of St Augustine’s College, Bristol: a transcript made by the antiquarian Thomas Smith (1638-1710) showed the date 1140 (with the 4 written ‘backwards’ in its twelfth-century form).<sup>179</sup> Apparently Wallis tried to confirm the inscription even as his book was nearly printed, for the final sentence of the ‘Emendations’ reads: ‘Having desired some to view it . . . they find the Inscription, but not the Date. Which therefore seems (by some accident) to have been defaced, since Dr Smith saw it there’<sup>180</sup> The subject continued to preoccupy Wallis into old age: in 1699 (aged 83) he annotated his own copy of the Latin translation of *A treatise of algebra* to the effect that he had asked Dr John Hall (Bishop of Bristol, 1691-1710) to look for the Bristol inscription but that it was no longer to be found.<sup>181</sup> In the same margin he noted a report from one Thomas Luffkin of Colchester about a window supposedly bearing the date 1090. Luffkin’s letter was published in the *Philosophical transactions* that same year, and a drawing of the window procured from him was printed the following year,<sup>182</sup> but there is no independent verification of this very early date, and it seems most likely that it was in fact 1494 with the ‘4’ written in the old looped style.

The Crusades from 1095 to 1270 took large numbers of Englishmen to the eastern Mediterranean where some of them must have learned the new

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<sup>179</sup> Wallis 1685a, 153.

<sup>180</sup> Wallis 1685a, 176.

<sup>181</sup> Wallis 1693, Savile Gg.2, 15.

<sup>182</sup> Luffkin 1699; Wallis 1700.

numerals, if only for the purposes of bargaining and trading. This could have been the origin of both the Helmdon and Bristol inscriptions, and it is curious that Wallis never considered the Crusades as important in this respect (see also § 4.30, § 4.31, § 4.33).

§ 4.27 Nor need it appear strange to any, that of this number 1133, the *Thousand* is expressed by *M<sup>o</sup>*, or the word *Millesimo* (of which that is an abbreviation). And only the latter part in Figures, 133; for that was (and still is) very usual. Thus in the Treatise of *Robertus Cestrensis* above mentioned, I find it thus written; *Annus namque Solaris in tercentum 65 dies atque unius diei quartam partem distinguitur*. And again: *Quibus executis, hos omnes dies in 30 multiplica, & multiplicationis summam per decem millia 631 divide*. (Where we have *tercentum 65*, for 365; and *decem millia 631*, for 10631.) and the like elsewhere. {See the Additions, pag. 153.}

In *Additions and emendations* Wallis also noted that the mixed use of words and symbols extended into early printed texts which often followed the conventions established in manuscripts.<sup>183</sup> In 1693 he pointed to yet further examples, from the *Musica* of Boethius and the *Astronomiae historia* of Ioan Stadius.<sup>184</sup> The two texts are bound together in the Savile Library and the *Musica* has been liberally annotated by Wallis with modern note names and sol-fa equivalents, evidence of the range of his interests and alertness of mind even in his late seventies.

§ 4.28 Since these things were written, I find in *P. Mabillon's* Treatise *De re Diplomatica*, (printed at *Paris*, 1681.) *Lib.II. Cap. XXV. §V.* mention made of a Bull of Pope *Stephen* the Ninth, (cited out of *Ughellus's Italia Sacra, Tom.I. col.465.*) thus dated: *Data anno Incarnationis MLVII Indictione XI*. With this Note of *Mabillon; Ubi pro XI ponitur II, vitio librarii qui pro Romanis numeris Arabicas ciphras male expressit*.

This is a clear example of Wallis's habit of adding new material to existing writing. Most of *A treatise of algebra* was composed by 1676 but the appearance of Jean Mabillon's *De re diplomatica* in 1681 caused Wallis to write additional paragraphs. The *De re diplomatica* was an enormous volume (47cm x 27 cm; 600 pages) in which Mabillon undertook to 'explain and illustrate the dates, materials and writing of ancient scribes, together with inscriptions and chronological notes as they pertain to the history, origins and

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<sup>183</sup> Wallis 1685a, 153; Censorinus 1503, 93, 94, 96, 111.

<sup>184</sup> Wallis 1693, 15; Boethius 1546; Stadius 1560, 14. Boethius and Stadius are bound together in Savile W.15.

learning of old times'. The second half of the book contains many fine full page examples of early styles of writing and, judging from the page wear on the Bodleian's copy, has been well used. Wallis, however, quoted an example from the first part, where Mabillon discussed the method of dating years from the Incarnation. Mabillon's note reads: 'where II has been written for XI, by an error on the part of the scribe who has represented Arabic figures badly as Roman numerals.'<sup>185</sup> Perhaps the most remarkable thing about this passage is that Wallis spotted it at all: it is not indexed and Wallis could only have found it if he was reading the text with considerable care.

§ 4.29 The words in Ughellus are thus: *Scriptum per manus Gregorii notarii & camerarii Sanctae Apostolicae sedis in mense Novembris die 19 indictione 2. Datum Romae 10 Kalendas Decembris per manus Humberti dicti Episcopi Silvae Candidae & Bibliothecarii Sanctae Romanae & Apostolicae sedis, anno Deo propitio 1057. Pontificatus Domini Stephani noni primo, inidct. 2.* Where Mabillon supposeth, that in the Original (or at least in some Copy whence this was taken) it had been written (in both places) *Indict. 11.* (in these Arabic figures) for *Eleven*; but the Transcriber (taking them to the Roman Numbers for *Two* expressed it by 2. And if indeed it were so in the Original, it is an argument that these Figures were then in use (though perhaps but rarely) in the year 1057: (Or at least in the year 1058, for so perhaps it might be written the Indiction for the year of our Lord 1057, being but 10; so that here seems to have been another mistake in the copying; where, for MLVIII, he puts 1057 instead of 1058, which might easily happen, if one of the three last strokes did in the Original begin with age to disappear; unless we chuse rather to say, that they did, at *Sept. 25.* begin to reckon a new Indiction, which was sometimes done, but not constantly, as Mabillon in that Chapter observed.) But this Argument is only conjectural, because we are not sure what it was in the Original.

Wallis with characteristic thoroughness went back to Mabillon's source, Ferdinando Ughelli's history of the church in Italy<sup>186</sup> and gave his own lengthy explanation of how 11 might have been changed to 2, and 1058 to 1057. His interest in calendrical matters and also, perhaps, his experience as *Custos archivorum*, would have made him especially sensitive to the question of when years began and ended and how they were named.

§ 4.30 And Mabillon himself takes no notice of it: For I find him there, *Lib.II. Cap. XXVIII. §X.* thus to speak: *Invenit [iuverit] hoc loco quaedam adicere de notis numericis, quae in consignandis Diplomatum calculis adhibitae sunt ab antiquis. Hae notae duplicis sunt generis: nempe Numeri*

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<sup>185</sup> Mabillon 1681, 184.

<sup>186</sup> Ughelli 1644-62, I, col 465.

*Romani & Arabici, quas vulgus cifras appellat. Recentior est harum cifrarum usus, quas Arabes ab Indis seculo X, Hispanos ab Arabibus seculo XIII, accepisse cum aliis censeat Athanasius Kircherus in Arithmologia sua (Part.I.Cap.IV.) Addit Papebrochius in Propylaei, num. 19. earum usum ante bella sacra usum non fuisse [minime notum fuisse Europaeis]. Ego vero nullum deprehendi ante seculum XIV. Thus Mabillon.*

§ 4.31 But for the Reasons above-mentioned, I take the use of them in *Europe* to have been much older than so: Not perhaps in the date of Charters and Legal Records, (for in such we find, even to this day, they are scarce admitted, our Lawyers, in their Records, constantly making use of the *Latin* Numbers, MDCLXVI;) but, at first, in Astronomical Tables, and Algorithmical Operations, and then by little and little in common use. And the *Arabs* I believe had them much earlier than the tenth Century.

This new quote from Mabillon appears in *De re diplomatica* some thirty pages after the previous one.<sup>187</sup> There are some misprints and inaccuracies in Wallis's transcription: in 1693 he changed *invenit* to *convenit* but the correct word is *iuverit*: corrections are shown in square brackets in § 4.30. The Latin passage translates as: 'It might help to add here something on the numerical notations which were used in seals on documents and in calculations from antiquity. These notations were of two kinds, Roman numerals and Arabic, commonly called ciphers. The more recent are the ciphers which, according to Athanasius Kircher in his *Arithmologia*, the Arabs received from the Indians in the tenth century and the Spanish from the Arabs in the thirteenth century. Papebrochius in Propylei, no. 19, adds that their use was very little known in Europe before the holy wars [Crusades]. I myself have detected nothing before the fourteenth century.'

Wallis's dismissive 'Thus Mabillon' was for him uncharacteristically curt. His scorn perhaps prevented him from taking seriously the idea that the 'holy wars' had indeed played some role in bringing the new numerals to northern Europe. For the most part, however, it was true that Mabillon's comments were of little value. The dates quoted from Kircher were at least two centuries too late,<sup>188</sup> and Mabillon's own observations were limited to diplomatic rather than mathematical use.

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<sup>187</sup> Mabillon 1681, 214.

<sup>188</sup> The Indian figures were known in the Islamic world by 760 AD and there is written evidence of them in Spain in 976 AD. See Hill G.F. 1915; Folkerts 1997, 4-6.

It was a pity that Wallis, having read the first part of *De re diplomatica* so thoroughly did not look more carefully at the illustrations in the second half. There he would have found an example he would surely have relished. Among the full page illustrations is an example of handwritten numerals of the 14<sup>th</sup>/15<sup>th</sup> century, from the Benedictine monastery described as *Cavensis*, probably of Cava near Salerno in southern Italy. The numerals are from chapter headings and run as follows:<sup>189</sup>

1	2	3	4	5	6	7	8	9
X	X1	X2	X3	X4	.	.		
XXX	XXX1	302	303	304	.	.		
XXXX	401	402						

The Arabic numerals in the third row are meant to be read as thirty-two, thirty-three . . This small example serves as a useful reminder of how slow, uneven and sometimes how painful the spread of Arabic numerals must have been.

§ 4.32 And (if I be not mistaken or misinformed) *Hermannus Contractus* (whom *Vossius* placeth about the year 1050, and Sir *Henry Savile* in a Manuscript of his, about 1040) was acquainted with them, and taught the use of them, in his time. But I think, his figures were in shape much different from those we now use, and said to be borrowed from some *Caldean* writer, and called by names of *Caldean Extraction*. But it is not the shape of the Figures, (which vary from day to day, as the shape of Letters also doth,) but the way or manner of using them, which we are now enquiring after. Of him I find mention in an ancient Manuscript in the *Bodleian Library*, That from *Hermannus* and *Prodocimus* they had learned the *Abacus*, which is another name for *Algorismus*. Nor were they then so well skilful in Oriental Languages, but that they might easily mistake a name, and write *Caldaean* for *Arabic* author.

Hermannus Contractus (d.1054) was a monk of the Abbey of Reichenau, now on the Swiss-German border, and was said to be frail in body, hence *contractus* or shrunken, but great in mind. Posterity regarded him as a linguist (Latin, Greek and Arabic), poet, historian, musician, philosopher, theologian and mathematician. He wrote a history of the world from the birth of Christ to the year of his death (it was continued by one of his disciples up to 1066), and some of his musical writing has also survived.<sup>190</sup> Two treatises, on the making and use of astrolabes, are ascribed to him, and if it was indeed he who wrote

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<sup>189</sup> Mabillon 1681, 373.

<sup>190</sup> Hermannus Contractus 1884; Migne CXVIII.

one or both, he was probably familiar with Arabic numerals; all the Bodleian Library copies, however, use Roman numerals.<sup>191</sup>

The description of the numerals of Hermannus as 'Caldean' was perhaps a reference to the unusual symbols, apparently of oriental origin, in Hermannus' musical writings. To support his case for Hermannus' knowledge of Hindu-Arabic numerals Wallis turned to methods rather than forms but his claim that *abacus* was another name for *algorism* as early as the eleventh century is false. At that time *abacus* would have been used only in the old way to mean a counting-board, not in the later sense of *abbacus* arithmetic (see § 3.19).

Prodocimus may have been the fifteenth-century Italian mathematician and astronomer Prodocimus de Beldomandis, but if so it seems strange that he was mentioned alongside Hermannus Contractus who lived four hundred years earlier. The manuscript which connects Hermannus and Prodocimus as teachers of the abacus, and which also presumably describes the numerals of Hermannus as 'Caldean', I have been unable to trace.

§ 4.33 Upon the whole matter therefore I judge, that about the middle of the eleventh Century, or between the year of our Lord 1000, and 1100, these Numerals Figures came into use amongst us in *Europe*, together with other *Arabic* Learning; first, on account of Astronomical Tables, and other Mathematical Books, and then by little and little into common practice.

This appeared to be Wallis's final thought on the matter. But before his book finally went to press he discovered, as he thought, evidence of even earlier use of the numerals, by Gerbert (later pope Sylvester II). Wallis did not say what prompted him to study the writings of Gerbert, but he read his letters, the *Epistolae Gerberti*, with considerable care.<sup>192</sup> He also consulted no fewer than five different accounts of Gerbert's life and work. As a result he wrote a long piece on Gerbert which he printed in *Additions and emendations* and instructed the reader to consider it inserted at the appropriate point in the main text (it was incorporated fully in the Latin translation in 1693).<sup>193</sup> For reasons

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<sup>191</sup> *De mensura astrolabii* and *De utilitatibus astrolabii*, Migne CXVIII, cols. 379-412. For Bodleian Library manuscripts see Bibliography.

<sup>192</sup> Masson 1611. The Bodleian Library catalogue lists a copy with shelfmark 8<sup>o</sup> G.16 Th. Seld, possibly the one that Wallis used, but the book cannot now be found.

<sup>193</sup> Wallis 1685a, 153-157; Wallis 1693, 16-18.



of length only the first four paragraphs are reproduced here, but they give a good indication of the meticulousness of Wallis's research:

§ But, upon further Search, I find the use of these Numeral Figures to have been yet Ancienter, even in these parts of the World.

§ And, in particular, I find that one *Gerbertus* or *Gerebertus*, was skilled therein; and brought the knowledge thereof, out of *Spain*, Into *France*, in the Tenth Century: As appears by divers passages in his Epistles extant, with this title *Gerberti Epistolae* published at *Paris* in the year 1611, (in Number 160.) with an account of his Life subjoined: and again in the year 1636. (in Number 161.) to which is added a second Collection, (in Number 55.)

§ He was bred a *Monk* at *Fleury* in *France*, (*Monachus Floriacensis*.) of the Order of *Benedictines*: (as appears Epist.70.) He was, after that, an Abbat; *Coenobii Bobiensis* (who were *Benedictines* also,) as we sometimes find it; or (as elsewhere) *Abbatiae Sancti Columbani* in *Italy*: As appears, Epist.2.3.4.5.12.14.18.24.83.130. But he oft complains of his ill usage there as Epist. 5.7.11.12.14.16.19.23.34.35.40.46.84.91.92.117.118.143. and elsewhere. He stiles himself *Scholaris* or *Scholasticus*, or *quondam Scholasticus*, epist.7.12.143.161.

§ He was afterwards (as we find in *Baronius* and others) Archbishop of *Rhemes* in the year 992; then of *Ravenna* in the year 996; and afterwards Pope of *Rome*, in the year 998, or 999; and so died in the year 1003. Whence that verse,

*Scandit ab R. Gerbertus ad R. post Papa vigens R.*

Which we find (with some little variation) in most of those that write of him.

The biographical details given by Wallis are roughly correct: Gerbert was a Benedictine monk of Aurillac, in France, and spent three years in northern Spain as a young man. On his return he became a tutor to the sons of both Otto I, Holy Roman Emperor, and Hugh Capet, king of France, and through their patronage, he became Archbishop of Rheims, then of Ravenna, and eventually the first French pope (Sylvester II) in 999. His reputation for learning became legendary, and he has been credited with being the first to introduce the Arabic numerals to northern Europe. It now seems, however, that he knew only the signs for the numerals, without the concepts of place value required for calculation. Gerbert's numerals, known as *apices*, appeared as abacus column

headings<sup>194</sup> but the actual calculations were written and performed using Roman numerals.

Wallis followed up a number of biographical accounts. Besides Baronio,<sup>195</sup> mentioned above, Wallis also consulted William of Malmesbury,<sup>196</sup> Vincent of Burgundy (or Beauvais, Bellvacensis, 1184-1264),<sup>197</sup> John Brompton (fl. 1436)<sup>198</sup> and Matthias Flacius (writing in 1567).<sup>199</sup> All of these accounts (except that of Baronio) were based on that of William of Malmesbury and repeated much the same tales of Gerbert as a practitioner of black arts and a conjurer of spirits, something he was said to have learned from the Saracens in Spain.<sup>200</sup> Only with the beginning of modern historical scholarship at the beginning of the seventeenth century did the tone of such biographies begin to change. Cardinal Baronio, attempting to redeem Catholic church history, described Gerbert as the worst pope that ever lived, but dismissed the more fanciful anecdotes as tales told by lamplight by simple girls to keep themselves awake (despite the fact they were all written and spread by men). Wallis was equally keen to distance Gerbert (and himself) from the taint of superstition, and commented that William of Malmesbury 'gave no great credit' to such tales, whereas in fact William and most of his successors wrote of little else. Wallis clearly wanted to establish Gerbert's credentials as a serious scholar and to this end he noted exactly which of Gerbert's 216 letters mentioned his interest in arithmetic. He ended his account of Gerbert with the following passage:<sup>201</sup>

§ Now that which makes me give the more undoubted credit to these writers (though a great while after,) as to his skill in *Algorism* or *Abacus* so early; is

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<sup>194</sup> For a reproduction of an eleventh century manuscript containing Gerbert's apices used as column headings see Ifrah 1998, 581.

<sup>195</sup> Baronio 1594-1603, X, 872-927.

<sup>196</sup> Savile 1596, 1-98; 36-37. Wallis used the 1601 edition, 6-174; 64-66. For a modern edition see Mynors 1998, 278-295.

<sup>197</sup> Beauvais 1591, caps 98-101.

<sup>198</sup> Twysden 1652, col 881.

<sup>199</sup> Flacius 1560-74, VI, cols 547-548; 659.

<sup>200</sup> Gerbert was supposed to have fashioned a magic head which could answer all questions. Similar tales were later associated with both Grosseteste and Bacon.

<sup>201</sup> Wallis 1685a, 157.

the concurrence of those passages which favour it, in his own *Epistles* as yet extant. For, otherwise, it is very possible (if nothing of this kind had appeared in his own writing, or of those who were his Contemporaries,) that those who should (after one or more Hundreds of year, when the names of *Abacus* and *Algorism* were come into use) write the History of *Gerbertus*, might (by a *Prolepsis* or Anticipation) make use of one or both of those Words; which, when they wrote were used for *Arithmetick*, to express his skill in Arithmetick, (though perhaps, not this kind of Arithmetick,) though the words were not known in the time whereof they wrote. But, finding the word *Abacus* (in this sense) more than once used in his own writings; there remains no scruple but that the thing was then in use, and known to him: and therefore as before we argued about the middle of the Tenth Century; and then, by him, brought into *France*, and known then to inquisitive Learned men (those especially who had to do with Astronomical Tables) though not yet into common use amongst the ordinary sort of men, and how much earlier yet it had before been known in *Spain* (amongst the *Moors* or *Saracens*) from whence he had it; doth not appear.

Unfortunately for Wallis's argument, Gerbert actually used *abacus* in its older sense, meaning a counting-board, not the new written methods. In his enthusiasm for Gerbert, Wallis was misled here into attributing to him far greater knowledge than in fact he could have possessed.

Nevertheless, despite errors and omissions, there can be no doubt that Wallis had achieved what he set out to do and had argued convincingly against Vossius' date of 1250 for the earliest arrival of the Arabic numerals in northern Europe. From his account a picture began to emerge, quite new in its time, of the slow and uneven spread of the numerals. From Wallis's research we can surmise that the numerals were partly known to a few scholars such as Gerbert and possibly Hermannus Contractus (as no doubt to travellers and traders) from the late tenth century. Inscriptions such as those at Helmdon and Bristol could have been the result of greater individual contact with Islamic culture during the Crusades (1095-1270) though Wallis rather surprisingly never suggested this. Only with the flood of new translations in the twelfth century did the numerals begin to appear more commonly in written texts, particularly astronomical tables. After 1200 the numerals were brought to England by Grosseteste and others, and together with the associated algorithms, were disseminated and popularised through the widely copied texts of Jordanus, Sacrobosco and Ville Dieu. Full acceptance of the numerals, however, was a slow and uncertain process; Roman and Arabic figures were used side by side, even mixed together, for hundreds of years, and Roman numerals have never entirely died out. Modern scholarship has, of course,

added a wealth of detail to this general overview. The development and spread of the Hindu-Arabic numerals was, as one might expect, a complex process: not only was there an inevitable reluctance to abandon the long established and easily understood Roman system, but there was also deep suspicion in some quarters of an eastern and non-Christian innovation. In its broad outline, however, Wallis's account has stood the test of time.

The two final paragraphs discussed the transition from manuscript to print.

§ 4.34 But the first (I think) who hath published any thing of this nature in print, is *Lucas de Burgo*, in *Italian*, in the year 1494; and after him (as *Buteo* informs us in his *Logistica*) *Stephanus a Rupe* in *French*, with whom *Stifelius*, in his *Arithmetic*, cites also *Adam Risen*, a *German*, (and all these, with their *Algorism*, treat also of *Algebra*;) For though *Hermannus Contractus*, *Prodocimus of Padua*, *Johannes de Sacro Bosco*, *Jordanus Nemerarius*, *Leonardo de Pisanus*, and others, had written thereof before; yet that was before Printing was in use: Nor do I know (though some other of their Works be yet extant,) that their Writings on this Subject have yet been printed, but are either not extant, or only in Manuscript.

*Lucas de Burgo* is better known as Luca Pacioli whose *Summa* of 1494 was among the earliest arithmetics to be published.<sup>202</sup> *Stephanus a Rupe* of Lyons was mentioned not only by *Buteo*<sup>203</sup> but also by *Gosselin*,<sup>204</sup> but has previously escaped identification.<sup>205</sup> It seems clear, however, that he was none other than Etienne de la Roche (*Stephanus*, Stephen, Etienne; *rupes*, rock or cliff, *roche*) who in 1520 published much of the work of Nicholas Chuquet. *Adam Ries* published a number of arithmetic books that taught both abacus techniques and the new Indian methods, and was greatly admired by *Michael Stifel* who cited him in his *Arithmetica integra*.<sup>206</sup>

Wallis was correct in supposing that in the seventeenth century none of the work of *Hermannus*, *Prodocimus*, or *Leonardo* had been printed: the music of *Hermannus* and the mathematics of *Leonardo* were published only in the nineteenth century. The work of *Jordanus* was partly published in the fifteenth century by Jacques Lefèvre d'Étaples (see § 4.17).

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<sup>202</sup> The first printed arithmetic was *Larte de labbacho*, Treviso 1478, see Smith D.E. 1987.

<sup>203</sup> *Buteo* 1559.

<sup>204</sup> *Gosselin* 1577.

<sup>205</sup> Van Egmond 1988, 141-142.

<sup>206</sup> *Ries* 1523; *Stifel* 1543.

Sacrobosco, on the other hand, was not only the first but the most widely published of the medieval writers. His *Algorismus* is known to have been printed at Strasbourg in 1488, again at Vienna in 1517, Cracow in 1521 and Venice in 1523. Wallis's next paragraph (§ 4.35) enables us to identify yet another early printed version, from 1503.

§ 4.35 Besides those above-named (and before most of them) is that of *Judocus Clichtoveus*, who in the year 1503 (and again in 1522,) published a Treatise of *Jacobus Faber Stapulensis* (whose Scholar he had been), entitled, *An Epitome or short Introduction into Boetius's Arithmetic*, with his own Commentary thereon. To which Treatise of Speculative Arithmetic, he subjoyns his own Treatise of Practical Arithmetic, or *Praxis numerandi, quem Abacum vocant*. And, to both these, one much more ancient (of an Author to him unknown), with this Title, *Opusculum de praxi numerorum, quod Algorismum vocant*. Of which last, I find an ancient Manuscript Copy in the *Savilian Library*, subjoyned to that Algorism of *Sacro-Bosco*, which I judge to be much of the same Antiquity with it, (about the year 1250, or sooner) and the most ancient of any yet printed; where we see, *Clichtoveus* useth both names, of *Abacus* and *Algorismus*, for this *Praxis numerorum*, by these Numeral Figures.

As Wallis described here, Josse Clichtove in 1522 republished the 1503 *Epitome* of Jacques Lefèvre d'Étaples (Jacob Faber Stapulensis) together with his own *Praxis numerandi*.<sup>207</sup> He also included the piece that he described in his preface as *opusculum de praxi numerorum* ('a small work on the practice of numbers'), an algorism *non inscite (nescio quo authore) compositus* ('not unskillfully written, whose author I do not know'). The piece opens with the words *Omnia quae a primeva rerum origine processerunt* which identify it as Sacrobosco's *Algorismus*. Wallis recognised the similarity to the text in the Savile Library (MS Savile 17, ff. 94<sup>v</sup>-104) but his sentence describing his find is confusing, for when he spoke here of 'that Algorism of *Sacro-Bosco*' in the Savilian Library he seems to have meant Ville Dieu's *Carmen de algorismo* which he had previously ascribed to Sacrobosco (See § 3.15.) The *Opusculum* was the true *Algorismus* of Sacrobosco, and precedes the *Carmen de algorismo* in MS Savile 17. Wallis failed to recognise the author of the *Opusculum*, but his observations enable us to add Paris 1503 to the list of Sacrobosco's publication dates.

Note once again Wallis's lack of distinction between abacus and algorism, whereas in Clichtove's book the words are used in quite separate contexts.

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<sup>207</sup> Lefèvre 1503; Clichtove 1503.

## Conclusion

For all its shortcomings Wallis's Chapter 4 is a noteworthy piece of original research, remarkable not only for its generally correct conclusions but for Wallis's use of a variety of historiographical methods. He examined not only the written contents of the texts at his disposal but also clues given by physical appearance; he made use of his extensive knowledge of mathematics, etymology and Classical languages, and drew on the expertise of others in oriental studies; and he consulted a wide range of secondary sources, from medieval English chronicles to works on music, astronomy and cryptography. His arguments were rarely over-stated and occasionally subtle, and where there gaps in his knowledge he was not afraid to say so. Wallis's account far surpassed that of Vossius which had led to it, not only in its conclusions but in its approach: the change in style and content in less than thirty years was little short of revolutionary.

During the seventeenth century, historiography, like every other study, changed rapidly. By comparing the approaches of Vossius and Wallis this chapter has explored just one facet of such change, the historiography of mathematics. After Wallis's work, mathematics could no longer be viewed in the old way as anciently revealed knowledge, sometimes lost, sometimes rediscovered, passed essentially unchanged from one civilisation to the next, but as a human endeavour influenced by culture and circumstance, in which ideas spread and took root in a complex variety of ways. Wallis, through his position in Oxford, was uniquely well placed to explore the history of mathematics in a new way, and like every historian, he owed much to those before him, in his case Leland, Bale, Allen, Bodley, Savile, Twysden, Vossius and many others, who from the mid sixteenth century onwards collected, recorded, preserved or published the legacy of medieval England. Above all he was indebted, as we are still, to the medieval writers themselves, both authors and copyists. The manuscripts which have survived in Oxford and elsewhere, with their exquisite penmanship, delicate illustrations, touches of humour and occasional unfinished pages are a moving testimony to those who, for all the harshness and unpredictability of their lives, struggled to comprehend their world and to share their insights with others

## Chapter 3

### How algebra was entertained and cultivated in Europe: from Leonardo of Pisa to Viète

#### Summary

Wallis dealt only briefly with late medieval and Renaissance algebra. For the period up to 1494 he had no primary sources. For the sixteenth century he had access to a wide range of printed books but was superficial in his reading of them so that he never fully appreciated the German and Italian contributions. However, in listing four names for algebra: *al-jabr*, *regula cosae*, *ars magna* and *analysis*, he unwittingly pinpointed the main trends in early European algebra. Using the sources Wallis had at his disposal, and others, this chapter attempts to distinguish the meanings behind these names, and re-evaluates Wallis's account.

In the thirteenth and fourteenth chapters of *A treatise of algebra* Wallis moved away from the history of the numeral system and took up the true theme of his book, the history of algebra. In two short chapters Wallis spanned four hundred and fifty years, and described how algebra was 'entertained and cultivated in Europe'<sup>208</sup> from the twelfth century, when the Arabic text *Al-jabr wa'l-muqābala* was first translated into Latin, to the end of the sixteenth century and the work of François Viète.

These years, approximately 1150 to 1600, were the vital formative period for European algebra but even Wallis could not claim that England was at the forefront of new developments, which came almost entirely from Italy and Germany. The lack of an early English school of algebraists meant that Wallis did not have access to the kind of manuscript evidence he had used so astutely in his account of the numeral system, and his knowledge of the evolution of algebra up to about 1500 was second-hand and at best sketchy. After the appearance of printed books Wallis was on firmer ground, for the Savile

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<sup>208</sup> *Treatise of algebra*, 64.

Library included (as it still does) a superb collection of algebra texts published throughout western Europe during the second half of the sixteenth century. Even with access to this rich vein of material, however, Wallis was more interested in what each writer said about his predecessors than in the contents of the texts themselves, and conspicuously lacking from his account is any discussion about what algebra was, or how it evolved. Yet it is possible to return to the texts Wallis had at his disposal and discover in them different strands of algebraic thought, each with its own literature and its own following. In order to evaluate Wallis's account, this chapter attempts to distinguish more clearly than he or some later authors have done between the various trends in European algebra up to the end of the sixteenth century.<sup>209</sup>

Wallis had already pinpointed the main aspects, almost without realising it, in his opening pages when he listed the names by which algebra had been known at different stages in the course of its evolution: *analysis*, *al-jabr*, *regula cosae* and *ars magna*. With his keen interest in word derivations he recognised the sources of these terms in Classical Greece, medieval Islam and Renaissance Italy respectively, but apparently regarded them as simply different names for the same subject. In fact each description carries its own meaning and emphasis, none of which is adequately conveyed by the single word *algebra*. In the course of this chapter it will be shown how each in turn came into use and what was understood by it, and against this background the strengths and shortcomings of Wallis's account will be more readily apparent.

### The names of algebra

*'In Arabic it is called al-jabr'* <sup>210</sup>

The text from which European algebra began to evolve and from which it eventually took its name was *Al-kitāb almukhtaṣar fī ḥisāb al-jabr wa'l muqābala* of Muḥammad ibn Mūsā al-Khwārizmī (b. before 800, d. after

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<sup>209</sup> For general recent accounts of the development of algebra in this period see Franci and Toti Rigatelli 1985; Van der Waerden 1985, 32-68; Parshall 1988; Pycior 1997, 10-39; Høyrup 1998; Bashmakova and Smirnova 2000, 49-90.

<sup>210</sup> *Treatise of algebra*, 2.



847),<sup>211</sup> a member of the scientific academy in Baghdad. The title is not easy to translate: *al-jabr* means *completion* or *restoration*, in the sense of re-setting a broken bone, and *al-muqābala* is *setting in opposition* or *balancing*. In al-Khwārizmī's text *al-jabr* generally means adding a positive term to eliminate a negative quantity while *al-muqābala* means balancing an equation by operating simultaneously on each side.<sup>212</sup> Perhaps, however, the terms originally referred to the traditional geometric technique of solving a quadratic equation by completing a square, so that *al-jabr* was the *completion* or *setting together* of the incomplete, or broken, square, while *al-muqābala* was the subsequent *balancing* which such completion requires. In time the terms used to describe this geometrical process would naturally transfer to the equivalent steps in the handling of the equation.<sup>213</sup>

The subject matter of al-Khwārizmī's treatise is the handling of linear and quadratic equations of the following types:<sup>214</sup>

squares equal to roots	$(ax^2 = bx)$
squares equal to numbers	$(ax^2 = b)$
roots equal to numbers	$(ax = b)$
squares plus roots equal to numbers	$(ax^2 + bx = c)$
squares plus numbers equal to roots	$(ax^2 + c = bx)$
roots plus numbers equal to squares	$(bx + c = ax^2)$

There is no symbolic notation in the treatise: the equations are described verbally, and the methods of solution are given as recipes for each case. The text gives geometrical demonstrations for some of the quadratics (a literal 'completion of the square') and there is then a long section of worked examples. The first few problems illustrate the six types of equation but the

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<sup>211</sup> Al-Khwārizmī's exact sources are not known, but Babylonian, Greek and Indian influences are evident in his work, see Toomer 1973, 360; Høyrup 1986; Høyrup 1993. See also Chemla 1994 for evidence that Islamic mathematicians drew on Indian and Chinese sources.

<sup>212</sup> Saliba 1973; Van der Waerden 1985, 4-5; Bashmakova and Smirnova 2000, 50.

<sup>213</sup> Høyrup 1986, 475-477; Høyrup 1993, 10-14.

<sup>214</sup> For a translation of al-Khwārizmī's text see Karpinski 1915, 66-157; chapters 1-6 of Karpinski's translation are reprinted in Grant E. 1974, 106-111.

remainder are concerned with mensuration and, at great length (almost half the book), with Islamic laws of inheritance and division.

The same material found its way into a slightly later text, *al-Kitāb fi al-jabr wa'l muqābala* of Abū Kāmil (c.850-930),<sup>215</sup> essentially an extension and commentary on al-Khwārizmī's *Algebra*. Abū Kāmil's work was taken up by al-Karajī (c.1010) and, eventually, in the west by Leonardo of Pisa, also known since the nineteenth century as Fibonacci.

*'The Italians have given it the name of Regula cosae'* <sup>216</sup>

The *Liber abbaci* (1202)<sup>217</sup> of Leonardo of Pisa was the first major European mathematical text, compiled from knowledge acquired by Leonardo on his travels through north Africa, Syria, Greece and Sicily. Through numerous practical problems and puzzles it helped to introduce and standardise the use of Indian numerals, but it also treated algebra by presenting problems which gave rise to quadratic equations, and set out the six equation types of al-Khwārizmī (whom Leonardo referred to as 'Maumeht') with examples. As with al-Khwārizmī, only positive solutions were considered but where two positive solutions existed Leonardo could produce both. He also treated equations of higher degree which were essentially quadratic in form, for example,  $x^8 + 100x^4 = 10000$ . Leonardo, like al-Khwārizmī, used verbal explanations and in doing so introduced the Latin names which were to remain in use for centuries: *numerus* for the 'constant' term, *radix*, *res*, *causa* or *cosa* for an unknown quantity, *quadratus* or *census* (literally 'wealth' or 'excess') for its square, *cubus* for its cube, *census de censu* and *cubus cubi* for the fourth and sixth powers respectively. (The fifth power was not used and not named.) Note that in this system powers are compounded by addition (the cube of a cube is the sixth power). Leonardo's exact sources are unknown, but he certainly used problems from al-Khayyāmī, Abū Kāmil, al-Karajī and al-Khwārizmī himself.<sup>218</sup> Al-Khwārizmī's text was translated into Latin twice

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<sup>215</sup> For translation and commentary see Levey 1966.

<sup>216</sup> *Treatise of algebra*, 3.

<sup>217</sup> The *Liber abbaci* was not published until the nineteenth century, see Boncompagni 1862, I.

<sup>218</sup> For a detailed comparison of Fibonacci and Abū Kāmil see Levey 1966, 217-220.

during the twelfth century, by Robert of Chester (c.1145) and by Gerard of Cremona (c.1175), and Leonardo might have known either or both.

The *Liber abbaci* began a tradition which lasted in Italy until the fifteenth century of *abbacus* texts devoted to the explanation and use of Hindu-Arabic numerals, commercial arithmetic and sometimes some algebra.<sup>219</sup> The earliest known treatment of algebra after Leonardo was by Jacob of Florence in 1307.<sup>220</sup> A few years later came the *Libro de ragioni* (1328) by the Florentine Paolo Gerardi<sup>221</sup> which, like the *Liber abbaci*, was mainly concerned with practical arithmetic, but in the last eight (of seventy) folios Gerardi explained the ‘Regolle delle cose’, the rules for handling an unknown ‘thing’ or quantity and gave al-Khwārizmī’s six equations with the rule and an example of each. He also added nine cases of cubic equations. Most, for example,  $ax^3 \pm bx^2 \pm cx = 0$  (in modern notation) are essentially quadratics (the solution  $x = 0$  would not have counted); others, of the form  $ax^3 = bx + c$ ,  $ax^3 = bx^2 + c$  and  $ax^3 = bx^2 + cx + d$  were solved incorrectly by the rules for quadratics. The fact that the so called solutions failed to fit the original equations did not prevent the rules being repeated in many later manuscripts up to and including the *Trattato d’abaco* by Piero della Francesca<sup>222</sup> in 1480.

The earliest text to be devoted entirely to algebra was the *Aliabraa-argibra* (1344) by Dardi of Pisa.<sup>223</sup> Where Gerardi had treated fifteen equation types, Dardi treated no fewer than 194, including some with fourth powers, all, however, reducible by a simple substitution to one of al-Khwārizmī’s six basic types. He also gave solutions to two special and easily solved cases of a cubic and a quartic arising from three or four years accumulation of compound interest.<sup>224</sup> Later authors repeated his rules for these cases but without stating the special conditions under which they applied.

One further manuscript from this period deserves special mention as the first attempt to explore the history of algebra. The *Trattato di prattica*

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<sup>219</sup> Franci and Toti Rigatelli, 1985; Van Egmond 1994.

<sup>220</sup> Karpinski 1929; Høyrup 1999.

<sup>221</sup> Van Egmond 1978; Franci and Toti Rigatelli 1985, 30-32.

<sup>222</sup> Franci and Toti Rigatelli 1985, 57-61; Smith F.K.C. 1994, 114-115.

<sup>223</sup> Franci and Toti Rigatelli 1985, 36-39.

*d'arismetica* (1463) by Benedetto of Florence described how algebra was brought to Italy not only through Leonardo's work but more directly through the Latin translations of al-Khwārizmī's *Algebra*.<sup>225</sup> Benedetto questioned the generality of some of the existing suggestions for solving cubic equations and so appears to have been a rather better mathematician than some of his predecessors. He also appeared to go further than most towards symbolic notation by using *r*, *c*, *b*, for *cosa*, *census* and *cubo*, then *cc* for *censo di censo* and *bb* for *cubo di cubo* but his scheme was not internally consistent as he also wrote *br* for *cubo relato cosa* (the fifth power), showing that these were abbreviations rather than systematic symbolic representations.<sup>226</sup> All the early Italian texts, like their Arabic predecessors, were rhetorical rather than symbolic: the common abbreviations *co*, *ce*, *p*, *m* for *cosa*, *census*, *plus*, *minus* were of the kind frequently used in manuscripts (and early printed texts) and were no more than precursors of a later true symbolism.

By the fifteenth century the teaching of Hindu-Arabic numerals and of algebra had spread to Germany, and it was there that the practice of solving equations came to be described as the cossick art (from the Italian *cosa*). It was also in Germany that the abbreviations *R*, *Z*, *C* (usually written in elaborate Gothic script) were introduced for *res*, *census* (or *zensus*), and *cubus*, and in combination (*ZZ* for the 4<sup>th</sup> power, *RZZ* for the 5<sup>th</sup>, *ZZZ* for the 6<sup>th</sup>, *CZZ* for the 7<sup>th</sup> etc) provided the first consistent symbolic representation of powers.<sup>227</sup>

The first printed text to include algebra was the *Summa de arithmetica* (1494) of Luca Pacioli, who chose to compound powers by multiplication rather than addition so that for him *cubus de censo* denoted a *sixth* power, not a *fifth*. In such a system a new name or symbol is required for every prime power, and Pacioli called his fifth power *primo relato*, the seventh *secondo*

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<sup>224</sup> Smith F.K.C. 1994, 119-124.

<sup>225</sup> Franci and Toti Rigatelli 1985, 51-52.

<sup>226</sup> Parshall 1988, 139-140.

<sup>227</sup> Nicholas Chuquet in his *Triparty* of 1484 used a system closely related to the modern index notation. His work was taken up and used by Etienne de la Roche, probably his student, but in his *L'arismetique* of 1520 de la Roche reverted to cossist notation and Chuquet's innovations were forgotten. See Flegg, Hay and Moss 1985.

*relato* and so on. The German author Christoph Rudolff in 1525 introduced the symbols  $\beta$ ,  $b\beta$ ,  $c\beta$ , . . for  $5^{\text{th}}$ ,  $7^{\text{th}}$ ,  $11^{\text{th}}$ , . . powers, a system which became standard during the sixteenth century. The precise symbols used varied from author to author: one of the earliest printed texts, the *Summa arithmetica* (1521) of Francesco di Ghaligai used charming but not very practical squares and rectangles to denote powers, but most authors used some variation on the letters R, Z, C for an unknown, its square and its cube.

Although an elementary symbolism was now in place, there had been little development of algebraic content since the *Liber abbaci* of Leonardo three hundred years earlier, or indeed from the *Algebra* of al-Khwārizmī almost four hundred years before that, and cossist texts continued to be written to much the same pattern throughout the sixteenth century. A typical text began by introducing the four operations of arithmetic and might also have included arithmetic treatment of fractions, powers and surds. Then the author would define the terms *cosa*, *census*, *cubus*, and set out his notation. Just as for numerical arithmetic he would show how to carry out the basic operations of addition, subtraction, multiplication and division in symbols and, in the more advanced texts, how to cancel simple fractions or extract easy roots. Next he would define an equation and show how it could be simplified: by moving terms from one side to the other (as taught by al-Khwārizmī) and by reducing the leading coefficient to 1 (also from al-Khwārizmī). Some authors also instructed the reader to divide out excess powers of the unknown (so that  $x^4 = 25x^2$  reduced to  $x^2 = 25$ ), to clear fractions by multiplication and surds by squaring. Then came the heart of the matter, the *Rule of Algebra* or the *Rule of Coss*: let the unknown quantity be represented by R, and form an equation according to the conditions of the problem; the solution to the equation is then the quantity sought. The transition from physical quantity to mathematical symbolism (and back) was seen as the key process, always emphasised on the printed page by a new heading or a special font. Here, just as with the term *al-jabr* in the Arabic texts, we see the word *algebra* used for a process of abstraction, of working with the underlying structure of a problem rather than its particular manifestations. Once the symbolisation was accomplished all that remained was to solve the resulting equations, and the six cases set out by al-

Khwārizmī provided the model, often followed (as in al-Khwārizmī's text) by worked examples demonstrating each case.

The exact contents and ordering of the material in the cossist algebras varied a little from author to author but what is most remarkable about these sixteenth-century texts is not their variety but their essential similarity over time and geographical distance. From Valencia to London and from Venice to Lyons, in Latin, German, Italian, Portuguese, French and English, a score or more of sixteenth-century authors wrote to much the same blueprint in a remarkable example of a common intellectual tradition. A full list of cossist algebras up to 1600 is given in Table 1, which also shows how many of these texts were available to Wallis as he wrote *A treatise of algebra*.

The best and most original was perhaps the *Arithmetica integra* (1544) of Michael Stifel which, in addition to the material outlined above gave a single rule that covered all cases of quadratic equations,<sup>228</sup> gave rules for working with negative numbers, and began to explore concepts of fractional and negative powers (Stifel observed that  $\frac{2187}{128}$  is what we would now write as  $(\frac{27}{8})^{2\frac{1}{2}}$  and that the multiplication  $\frac{1}{8} \times 64 = 8$  corresponds to what would now be written  $2^{-3} \times 2^6 = 2^3$ ). Many later writers acknowledged a debt to Stifel, and the output of new books peaked during the following decade, with works by Scheubel, Peletier, Buteo and Ramus published in France,<sup>229</sup> Aurel and Perez de Moya in Spain,<sup>230</sup> Mennher and Nuñez in the Netherlands,<sup>231</sup> and Peucer in Germany.

The single English representative of the genre was Robert Recorde's *The whetstone of witte* (1557). Recorde had already published an arithmetic, *The grounde of artes*, in 1543, so *The whetstone* was devoted more particularly to algebra. As with all his books there is delightful wordplay in the title: not only

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<sup>228</sup> Stifel's rule was as follows: halve the coefficient of the root, square it, add or subtract the 'number', take the square root of the resulting quantity, add or subtract half the coefficient. This led to the mnemonic AMASIAS. It was nearly 90 years later that Oughtred in England gave the first general *formula* for the solution of quadratic equations.

<sup>229</sup> Van Egmond 1988; Cifoletti 1996, 128-140.

<sup>230</sup> I have not yet found any copy of Perez de Moya's *Arithmetica*.

<sup>231</sup> Mennher wrote in French, Nuñez in Portuguese, but both published in Antwerp.

was the *whetstone* to be found in the previously prepared *grounde* ('The grounde of artes did brede this stone') but the Latin for whetstone is *cos*. Recorde's text is written in the form of a dialogue between master and pupil and is lively and thoroughly readable. It is perhaps best known for Recorde's introduction of the equals sign: 'I will lette as I doe often in woorke use, a paire of paralleles . . bicause noe .2. thynges can be moare equalle.' Here is Recorde's teaching on the *Rule of Algebra* (typeset as closely as possible to the original):<sup>232</sup>

*The rule of equation, commonly called Algeber's Rule*

. . But now will I teache you that rule, that is the principall in Cossike woorkes and for whiche all the other dooe serve. This rule is called the Rule of Algeber, after the name of the inventours, as some men thinke . . But of his use it is rightly called, the rule of *equation*: bicause that by *equation* of numbers, it doeth dissolve doubtfull questions: And unfolde intricate ridles. And this is the order of it.

*The somme of the rule of equation:*

*When any question is propounded apperteinyng to this rule, you shall imagin a name for the number, that is to bee soughte, as you remember, that you learned in the rule of false position. And with that number shall you procede, accordyng to the question, until you find a Cossicke number, equalle to that number, that the question expresseth, whiche you shall reduce ever more to the leaste numbers . .*

*The whetstone* was the only sixteenth-century English text dedicated solely to algebra. A little algebra was taught in the *Stratitocos*, essentially a military

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<sup>232</sup> Recorde's text is unpaginated. This extract is from the section entitled 'The arte of cossicke numbers'.

manual, of Leonard and Thomas Digges, published in 1579, but Wallis later searched in vain for any other English treatment of the subject.

Elsewhere in western Europe, cossist algebras continued to appear well into the seventeenth century.<sup>233</sup> The later texts, however, lack the freshness and vigour of their mid sixteenth-century predecessors and are often ponderous or confused. Cossist symbolism at first seemed to carry some promise but always carried the seeds of its own decline, for it was little more than a system of abbreviations, incapable either of generalisation or of true symbolic manipulation. It was adequate, just, for handling the linear and quadratic equations set out by al-Khwārizmī, but the sixteenth-century cossist writers made little or no attempt to extend their work to cubics, and the notation shed no light on the structure or nature of such equations. Under the weight of new demands such a system was bound to collapse.

Before its lingering death in the seventeenth century cossist algebra taught European mathematicians important lessons about operating with symbols instead of numbers: all the cossist writers saw the rules for manipulating symbols as direct parallels to the corresponding rules for numbers (Recorde was not alone in calling his symbols ‘Cossicke *numbers*’). The notation may have been flawed but cossist algebra was the beginning of a generalised symbolic arithmetic. In technique as opposed to symbolism, however, the cossists made no advance. A student of Leonardo’s algebra would have discovered nothing essentially new in any of the later books, and even a reader of al-Khwārizmī would have found much that was still recognisable. The Arab-cossist tradition lasted for eight centuries without extending either its scope or its methods, until eventually it ran into the ground. Fortunately for the future of mathematics, cossist algebra was not the only strand in the story; in mid sixteenth-century Italy something entirely new was under way.

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<sup>233</sup> For instance, Clavius 1608 (Germany), Cataldi 1618 (Italy), Cyriaque de Mangin 1623 (France) and others. Cossist notation occasionally appeared up to thirty or forty years later in, for example, Renaldini 1655; Brasser 1663.



*'Cardan gives it the name of Ars magna'* <sup>234</sup>

Pacioli stated in the *Summa* that it was impossible to solve a cubic equation by the kind of rules which worked for quadratics.<sup>235</sup> This is usually read as a statement of failure but should rather be seen as a rejection of the erroneous methods proposed by Gerardi and others and a first important step forward in the understanding of cubic equations. Pacioli also solved a special case of a quartic equation by treating it as the product of two (identical) quadratics, foreshadowing later more general techniques for quartics,<sup>236</sup> but the main advances came a few years after his death in 1517. The first mathematician to succeed with equations of the form  $x^3 + px = q$  was Scipione del Ferro (1465-1526), lecturer in geometry and arithmetic at the University of Bologna. He never published his result but taught it to his son-in-law, Annibale della Nave and to his disciple Mario Fiore. In 1535 Niccolo Tartaglia (c.1499-1557), in response to a challenge from Fiore, discovered for himself del Ferro's solution and four years later passed the secret on to Girolamo Cardano (1501-1576) on condition, he later complained, that Cardano would not publish it. Cardano, however, discovered (through Nave) del Ferro's priority and published the result along with many others, and with full attribution to his predecessors, in the *Ars magna* of 1545.<sup>237</sup>

The *Ars magna* is one of the great mathematical texts of all time. Del Ferro's particular solution is only one of a host of important discoveries the book contains. From del Ferro's starting point Cardano not only worked out the solution to every case of cubic equation but, together with his pupil and son-in-law, Ludovico Ferrari, tackled quartics too. Unlike the cossist texts, however, the *Ars magna* is more than a manual for solving set forms of equations. It contains profound and far-reaching insights which were not to be fully worked out for fifty years or more and which truly laid the foundations of modern algebra. As Cardano's biographer Ore has written: 'some of the more

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<sup>234</sup> *Treatise of algebra*, 3.

<sup>235</sup> Pacioli 1494, I, dist. VIII, tractate 5.

<sup>236</sup> Smith F.K.C. 1996.

<sup>237</sup> Cardano 1993, xviii-xxii.

visionary mathematicians, especially Cardano, began to see the general principles which were to occupy mathematicians in the centuries to come.<sup>238</sup>

The *Ars magna* is not easy reading even now. It was written in long winding sentences without any use of notation, and some of the vocabulary is both specialised and obscure so that even in modern translation the meaning is not always plain. Cardano presented a plethora of rules and methods, some of which he illustrated by examples but he rarely gave any explanation of how he found them. He gave occasional geometrical demonstrations but they have the appearance of being appendages rather than integral parts of the text, and they disappear altogether from the later part of the book. The following account of the contents will focus on those aspects which were of key importance to the later development of algebra.

The contents of the first chapter alone display the originality and fertility of Cardano's insights. He began with cubic equations<sup>239</sup> of the form  $x^3 + px = q$  and  $x^3 + q = px$  (no term in  $x^2$ ) and by comparing  $\frac{2p}{3} \times \sqrt{\frac{p}{3}}$  with  $q$  was able to determine not only the number of real roots of each equation but how many were 'true' (positive) or 'fictitious' (negative).<sup>240</sup> This in itself was an astonishing advance: up to that time, and for another fifty years to come, most writers steadfastly ignored the possibility of negative roots. He went on to carry out the same kind of analysis for three further classes of equation:

no term in $x^3$	$(x^2 + px = q \text{ or } x^2 + q = px)$
no term in $x$	$(x^3 + px^2 = q \text{ or } x^3 + q = px^2)$
all possible terms present	$(x^3 + r = px^2 + qx \text{ etc})$

Cardano noted that by changing the sign of certain coefficients (or in his layout changing side, since he kept all his coefficients positive) a 'fictitious' root could be changed to a 'true' root. For example. The 'fictitious' root (-3) of

$$x^3 + 72 = 6x^2 + 3x$$

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<sup>238</sup> Ore 1953, 47-48.

<sup>239</sup> For clarity Cardano's equations and results are given in modern notation but his own exposition was always verbal.

<sup>240</sup> The quantity  $4p^3 - 27q^2$  serves as a 'discriminant' for cubics as ' $b^2 - 4ac$ ' does for quadratics.

becomes the ‘true’ root (+3) of

$$x^3 + 6x^2 = 3x + 72$$

Such observations laid the foundations of the ‘Rule of Signs’ proposed by Descartes almost a century later.

The next few chapters treated well known material: equations of the form  $ax^n = bx^m$  and the three types of quadratics, but then in Chapter 6, entitled *De modis inveniendi capitula nova* (‘On methods for solving new cases’) Cardano took off into the unknown. ‘After I had carefully considered all this’, he wrote, ‘it seemed to me that it would be permissible to go still further’.<sup>241</sup> He started simply enough, by comparing  $x^8 + x^4 = 12$  with  $x^2 + x = 12$ ; such ‘higher quadratics’ had already been explored by Leonardo. But a few paragraphs later<sup>242</sup> he came up with what was perhaps the most profound insight in the whole of sixteenth-century mathematics. It arose from the problem of finding two numbers here, denoted by  $p$  and  $q$ , such that

$$p + q = q^2$$

$$pq = 8$$

The substitution  $q = 8/p$  leads to

$$p^3 + 8p = 64 \quad (1)$$

The substitution  $p = 8/q$ , however, leads to

$$q^3 = q^2 + 8 \quad (2)$$

In the *Ars magna* both equations are described in terms of a single ‘unknown’ so it is closer to Cardano’s text to express (1) and (2) in terms of a single variable  $x$ :

$$x^3 + 8x = 64 \quad (1)$$

$$x^3 = x^2 + 8 \quad (2)$$

The equations are clearly different in form yet their roots are related in a simple way. Thus, Cardano reasoned, it was possible to transform one equation to another by a simple change of root, and therefore equations of type (1), which he could solve, opened up equations of type (2). As Cardano put it: ‘Translate questions that are by some other ingenuity known, to questions that

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<sup>241</sup> Cardano 1993, 48.

<sup>242</sup> Cardano 1993, 51-52.

are unknown, and the discovery of rules will have no end.'<sup>243</sup> Cardano was right: the transformation and solution of equations by change of root was to be a central part of the work of Viète and Harriot around 1600, but far beyond that, investigations into the nature and solvability of equations in relation to properties of their roots were eventually to become the starting point of modern abstract algebra.<sup>244</sup>

Cardano himself went on to explore the above and other transformations and so solved thirteen different cases of cubics. In the penultimate chapter of the *Ars magna* he also expounded the method devised by Ferrari for solving quartic equations: essentially a method of 'completing the square' by the introduction of a new quantity (though Cardano had no notation to distinguish between the first and second unknowns). The condition for a perfect square led to a cubic in the second unknown, which in principle could be solved, and the value obtained led back to the solution of the original quartic.<sup>245</sup> The mathematical skill required to carry out this process, without any notation to assist either the conceptualisation or the description, is of the highest order. Little wonder that Cardano wrote of it:<sup>246</sup> 'And therefore carrying out such operations as these is about the greatest thing to which the perfection of human intellect or, rather, of human imagination, can come.'

The lack of notation is one of the most remarkable features of the *Ars magna* and raises profound questions as to the true nature of algebra. Most mathematicians from the cossists onwards have regarded symbolic operation as an essential feature of the subject, yet the cossists for all their symbolism made few conceptual advances, while Cardano with no notation at all displayed astonishing imagination and inventiveness. The *Oxford English dictionary* definition of algebra as a 'branch of mathematics that uses letters etc. to represent numbers and quantities' was already inadequate in 1545,

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<sup>243</sup> *Quaestiones igitur alio ingenio cognitae ad ignotas transfere positiones, nec capitulorum inventio finem est habitura, . .*

<sup>244</sup> Van der Waerden 1985; Toti Rigatelli 1994.

<sup>245</sup> Fully worked examples of the method may be seen in the *Ars magna* at Chapter 39 amongst problems V to XIII, Cardano 1993, 239-253.

<sup>246</sup> Cardano 1993, 246.

whereas Cardano's vision of algebra as the investigation of mathematical structures and their relationships is much closer to the modern understanding.

The originality of Cardano's insights combined with the obscurity of his writing meant that in its own day and for a long time afterwards the *Ars magna* was highly regarded but little understood. The cossist writers sometimes mentioned it but continued with their own set pieces. Cardano found no champion until his work was taken up by another Bolognese mathematician, Rafael Bombelli (1526-1572), who recognised the value of Cardano's work but wanted to make the exposition clearer. In this he succeeded: Book II of his *L'algebra* written between 1557 and 1560 is a lucid and systematic treatment of quadratic, cubic and quartic equations by Cardano's methods. Bombelli introduced, for the first time in a printed text, a form of index notation in which the equation  $x^3 = 6x + 40$  appeared as  $1^{\text{3}} \text{ a. } 6^{\text{1}} \text{ p. } 40$ . His book is perhaps most famous, though, for his use of imaginary quantities which he called *piu di meno* and *meno di meno* (for  $+\sqrt{-n}$  and  $-\sqrt{-n}$  respectively). By adding what would now be called complex conjugates he was able to show that an apparently 'impossible' quantity was in fact real: for example,  $\sqrt[3]{2 + \sqrt{-121}} + \sqrt[3]{2 - \sqrt{-121}} = (2 + \sqrt{-1}) + (2 - \sqrt{-1}) = 4$ . Finding the cube root of a complex quantity is a non-trivial matter and Bombelli's examples were carefully chosen, but they enabled him to solve certain cases of 'irreducible' cubics.

Part III of *L'algebra* originally contained a series of practical problems chosen to demonstrate the principles of algebra. However, after studying and partly translating a manuscript of Diophantus' *Arithmetica* in the Vatican Library, Bombelli re-wrote this section entirely and included problems of a more abstract nature, many taken directly from Diophantus.<sup>247</sup> Through Bombelli's *L'algebra* and Xylander's translation of the *Arithmetica* in the same year<sup>248</sup> the work of Diophantus began to be more widely known and was to play an important role in the later development of algebra and number theory.

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<sup>247</sup> Reich 1968; Jayawardene 1973.

<sup>248</sup> Diophantus 1575.

Another important book which presented and explored Cardano's work more clearly was *L'arithmetique . . . aussi l'algebre* (1585) of Simon Stevin (1548-1620) who, like Bombelli, was by profession an engineer. Stevin gave a brief and accurate historical introduction to the subject of solving equations, ending with Bombelli, 'a great arithmetician of our time'. He took up Bombelli's notation for powers of the unknown (except that he used full instead of half circles) and in theory extended it to fractional indices: in theory only, because although he described the meaning of encircled  $\frac{1}{2}$ ,  $\frac{2}{3}$  etc, such symbols never appeared in print, either because they were beyond the capability of contemporary typesetting or because they were not actually needed in a book dealing with polynomial equations. Bombelli and Stevin both made important innovations in notation but arguably their greatest contribution to algebra was to publish thorough and systematic treatments of Cardano's methods, thus making his ideas more easily available to a new generation of algebraists: Viète (to be discussed below), Harriot and Descartes.

Detailed consideration of the contents of the *Ars magna* by historians of mathematics has been rare, the most notable exception being Charles Hutton's account in 1796.<sup>249</sup> A modern article by Mario Gliozzi also lists the full range of Cardano's innovations.<sup>250</sup> Many modern commentators, however, have focused almost exclusively on Cardano's treatment of cubic and quartic equations and in this respect some recent authors have regarded his work as less than wholly original.<sup>251</sup> There was, of course, nothing new about tackling cubic and quartic equations; where Cardano broke new ground was not in

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<sup>249</sup> Hutton 1796, 68-73; Hutton 1812, 206-224. The sections on Cardano are the same in each article.

<sup>250</sup> Gliozzi 1971, 65; see also Smith F.K.C. 1999.

<sup>251</sup> Parshall 1988, 143, 149; Pycior 1997, 10, 12. Parshall discusses Cardano's work in the context of 'natural selection of ideas' in a mathematical environment, an approach which perhaps begs more questions than it answers about how such an environment is created. Pycior, searching for the background to nineteenth-century symbolic algebra, dwells almost exclusively on the acceptance or otherwise of negative and imaginary numbers. In both cases the need to support a particular theory or point of view has perhaps hindered the appreciation of Cardano's innovations from a sixteenth-century perspective.

attempting such problems but in the methods he devised. In particular his insight into the way equations could be transformed by a change of root, either linear ( $x' = x - k$ ) or reciprocal ( $x' = k/x$ ) was unprecedented. Perhaps one of the most perceptive readers of the *Ars magna* was John Pell who in 1638 singled out the crucial Chapter 6 in which Cardano first announced his breakthrough on what Pell, following Cardano, called the *transmutation* of equations. Pell's translation of this chapter into English survives among his unpublished papers<sup>252</sup> and until recent years was, as far as is known, the only rendering of any part of the *Ars magna* into English.<sup>253</sup> Pell was thoroughly familiar with the mathematical literature of his day, including the algebra of Viète, Harriot and Descartes, and would have recognised, perhaps more clearly than some later writers, that Cardano's insights were the foundation of all subsequent progress in the theory of equations for almost a century.

*'What we commonly call algebra is by a Greek name called Analysis'* <sup>254</sup>

Beginning in 1590 François Viète (1540-1603)<sup>255</sup> took algebra into the third and last of its sixteenth-century definitions. What separated Viète most markedly from his predecessors was the new availability of Greek sources, and the distinguishing feature of his work was not, as is commonly supposed, any major advance in notation or technique, but his application of algebra to the work of Greek writers, particularly Pappus and Diophantus. Bombelli had already treated the arithmetic problems of Diophantus, but Viète took algebra into a different field, geometry. His purpose in doing so was to recover the lost theorems of Classical geometry through algebraic analysis,<sup>256</sup> and so algebra became 'the analytic art', and thus acquired the name by which it was to be commonly known for much of the seventeenth century.

Pappus had spoken of two types of analysis: the 'poristic', a method of proof which works from the proposition to be proved through its consequences

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<sup>252</sup> British Library Add MS 4409, f. 261-261<sup>v</sup>.

<sup>253</sup> Pell translated the key sentence in note 243 as: 'Therefore transfer to unknown positions, questions otherwise known, y<sup>e</sup> invention will be endlesse . . '

<sup>254</sup> *Treatise of algebra*, I, opening sentence.

<sup>255</sup> For Viète's life and work see de Morgan 1838, XXVI, 311-318; Busard 1975.

<sup>256</sup> For an excellent description of Viète's aims and methods see Mahoney 1973, 26-48.

to a known truth and the ‘zetetic’, used for finding a solution by supposing it known and then establishing the necessary relationships. The second was used by Diophantus throughout his *Arithmetica* and also corresponds to the *Rule of Algebra* of the cossist texts. Viète added a third kind of analysis, the ‘rhetic’ or ‘exegetic’ in which an unknown magnitude (arithmetic or geometric) is actually produced from an equation (corresponding to the cossist rules for solving equations).

Viète’s algebra is a rich blend of the work of his predecessors. His earliest book on the subject, *In artem analyticen isagoge* (1591),<sup>257</sup> in many ways continued the cossist tradition: the rules for the four operations of arithmetic with symbols were set out at great length but now under a classical heading, *logistice speciosa* (calculation in ‘species’, or ‘types’). The concept of *zetetic* was the equivalent of the cossist *Rule of Algebra* and Viète’s *leges zeteticae*, or rules for handling equations, were precisely those set out by Arabic and cossist writers but renamed in Greek by Viète as *antithesis* (moving terms from one side to the other), *hypobibasmus* (removing excess powers of the unknown) and *parabolismus* (reducing the leading coefficient to 1). By such names Viète identified, and distinguished between, existing procedures but did not add to them. In Viète’s next algebra text, *De aequationum recognitione*, written in 1593 (but not published until 1615) the methods and results of the *Ars magna* were everywhere apparent, particularly in the methods of transforming equations and in the solutions of cubics and quartics. Viète’s theoretical exposition owed almost everything to Cardano but he also took a practical approach to equation solving and introduced the numerical technique which had been well developed by Arab mathematicians and which was to be refined into its modern form in the nineteenth century.<sup>258</sup>

Viète made limited advances in notation by introducing  $A_{\text{quadratus}}$ ,  $A_{\text{cubus}}$  for A squared, A cubed, thus for the first time denoting not only the power but also the base quantity, A, (he always used vowels for unknown quantities). However, the retention of verbal terms such as *quadrato-cubus* meant that his system retained both the ambiguity and lack of generalisability of the cossist

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<sup>257</sup> Viète 1591; for modern translations see Viète 1968 and Viète 1983.

<sup>258</sup> Rashed 1974.



notations and so was less useful than either Bombelli's or Stevin's. In addition Viète was concerned to retain dimensional homogeneity and achieved it only by introducing artificial devices such as  $Z_{\text{plano}}$  (to indicate a two dimensional quantity) which render his notation heavy and cumbersome. His greatest notational advance was perhaps the use of letters (non-vowels) for known but unspecified quantities, an important step towards generalisation, but he still used verbal links such as *B in A* to indicate multiplication and *aequitur* for equality, so his work has none of the clarity of modern algebra, and is considerably harder to read than Stevin's a few years earlier or Harriot's a few years later.

Viète is often considered the founder of modern algebra but in his theoretical writing he is more correctly seen as heir to Cardano, while in his treatment of Diophantus he was preceded by Bombelli.<sup>259</sup> His most important contribution to algebraic thinking was perhaps his insight into the way algebra could open up geometry, in contrast to all previous authors who (recall al-Khwārizmī and Cardano) had used geometry to justify algebra. In this Viète laid important foundations for the work of Descartes, and therefore stands as a transitional figure both chronologically and mathematically between the sixteenth and seventeenth centuries. By 1600, thanks to Cardano and Viète, the ideas which would dominate algebra for the greater part of the new century, the structure of equations and the application of algebra to geometry, were in place. The Arabic origins of algebra could still be traced in its name but in every other way modern algebra was a thoroughly European and sixteenth-century invention.

### **Wallis's account of medieval and Renaissance algebra**

Of Wallis's two chapters on medieval and Renaissance algebra the first (Chapter 13), 'Of Leonardus Pisanus, Lucas Pacioli, Cardano, Tartalea, Nunnes, Bombel and other writers of algebra before Vieta', deals with European writers up to the end of the sixteenth century. The second (Chapter 14), 'Of Francis Vieta and his specious arithmetick', is devoted to Viète alone. The titles themselves betray Wallis's perspective: despite his original research

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<sup>259</sup> Viète 1593a; Reich 1968; Jayawardene 1973.

on the number system he has now returned to viewing mathematical history as a succession of authors rather than the development of ideas. As has already been mentioned, Wallis lacked primary sources for the early part of this period but had excellent resources at his disposal for the sixteenth century, and it is interesting to see how much, and sometimes how little, he used them.

Wallis knew that algebra had come to Europe from Arabic sources along with other mathematical texts on arithmetic and astronomy, but he had to admit that he had never seen an Arabic text on algebra nor, presumably, any Latin translation of such texts for none had reached Oxford.<sup>260</sup> In fact the first translation of al-Khwārizmī's *Algebra* was made by Robert of Chester, whose identity Wallis had researched with some care, but without ever discovering that Robert had translated this key text, so he never knew that algebra was first brought to northern Europe by 'one of ours'.

Wallis had never seen the *Liber abbaci* either. He could not report on its contents, and he dated it two centuries too late, at 1400; in this he was following Vossius<sup>261</sup> who, like Wallis, knew of the *Liber abbaci* only indirectly. Wallis knew nothing of the fourteenth- and fifteenth-century Italian *abbacus* texts, and his account really begins with Pacioli's *Summa*.<sup>262</sup> This he described in greater detail than any other early text, noting both its mathematical contents and the writers Pacioli claimed as his sources (Leonardo of Pisa, Prosdocimus, Jordanus, Sacrobosco). Wallis drew particular attention to the fact that all the algebra in the *Summa* (which went as far as quadratic equations) was of Arabic origin with no trace as yet of Greek sources.

The next author mentioned by Wallis was Francesco di Ghaligai (Wallis called him Caligarius) whose *Summa di arithmetica* containing chapters on *Arcibra* was published in Italian in Florence in 1521. Wallis again quoted Vossius who gave a date of 1515, and Wallis appeared not to have seen the

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<sup>260</sup> Karpinski 1915, 49-63; Hughes 1982; Levey 1966, 7-11. Wallis's claim that Arabic algebra was known in Europe before Leonardo was still being disputed in the nineteenth century, see Colebrooke 1817, li.

<sup>261</sup> Vossius 1650, 314.

<sup>262</sup> The Bodleian Library owns a 1523 edition of the *Summa* printed at Toscolano.

book himself. The 1521 edition of Ghaligai's *Summa* now in the Bodleian Library is a later acquisition which Wallis could not have seen,<sup>263</sup> but there is a 1552 edition in the Savile Library which he appears to have missed.<sup>264</sup>

Of Michael Stifel's *Arithmetica integra* (1544) Wallis had little to say except to mention the notation, and to note Stifel's reference to the earlier authors Adam Ries<sup>265</sup> and Christoph Rudolff.<sup>266</sup> On Cardano there was barely more; again Wallis's main interest was in who Cardano cited as his sources: al-Khwārizmī (whom Cardano called Mahomet Filius Mosis), Leonardo of Pisa, Pacioli, Ferro, Tartaglia and Ferrari. Of the content of the *Ars magna* he mentioned only the rules for solving cubic equations and remarked (as others have done since) that the rules were 'by him first Published, though not first invented'.<sup>267</sup> Elsewhere he complained that he found Cardano's demonstrations 'intricate and perplexed',<sup>268</sup> and it seems that he never actually read the later pages of the *Ars magna*, for he believed to the end of his life that Bombelli was the first mathematician to treat quartic equations. Pell, a more astute reader of Cardano, and possibly the only person who could have corrected Wallis, had died by the time Wallis published his remarks. Given Cardano's reputation and the easy availability of his work in Oxford, Wallis's ignorance of his innovations is inexcusable and perhaps the most serious lapse of his entire history.

Wallis next made brief references to the works of Scheubel,<sup>269</sup> Peletier,<sup>270</sup> Buteo (Borrell)<sup>271</sup> and Nuñez,<sup>272</sup> all of which were available to him in the

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<sup>263</sup> Ghaligai 1521 [Don.e.283].

<sup>264</sup> Ghaligai 1552 [Savile M.17] bound with Peletier 1554 but not listed in Bernard 1697. Ghaligai 1552 is entitled *Practica d'arithmetica* but apart from some changes in notation the text is essentially the same as Ghaligai 1521.

<sup>265</sup> Ries wrote his *Coss* in 1523 and revised the manuscript in 1539 but it was never published though other works by Ries did appear in print.

<sup>266</sup> Rudolff 1525.

<sup>267</sup> *Treatise of algebra*, 62.

<sup>268</sup> Wallis to Collins 12 April 1673, Rigaud 1841, II, 573.

<sup>269</sup> Scheubel 1551.

<sup>270</sup> Peletier 1554 [Savile M.17].

<sup>271</sup> Buteo 1559 [Savile Cc.11].

<sup>272</sup> Nuñez 1567 [Savile Aa.7].

Savile or Bodleian Library. Bombelli's *L'algebra*, surprisingly, seems never to have been in the Savile collection though there is a copy in the main Bodleian Library.<sup>273</sup> Wallis noted only that Bombelli treated quartic equations, information he might have gleaned from his reading of Stevin. Passing mention of Ramus,<sup>274</sup> Salignac,<sup>275</sup> Stevin,<sup>276</sup> Clavius,<sup>277</sup> and Henisch,<sup>278</sup> all except the last in the Savile Library, brought Wallis into the early years of the seventeenth century. There had been further similar writers since, he observed, but none had gone beyond quadratic equations.

Wallis was aware of the lack of English names in his list and tried, somewhat desperately, to remedy the defect. He came up with:<sup>279</sup>

*Leonard Digges* (in his *Stratoticos*, 1579,) and *Robert Record*, about the Year 1552, (if I be not mis-informed,) and (I think) Robert Norman, about 1560, and some other, (whose Names I do not remember,) have written of it in our own language.

The meagreness of this list obviously troubled Wallis for when he sent his draft to Collins in 1677 he wrote: 'You may mind me also of the names of ancient algebraists of our own before Vieta. Such I have seen, but have forgot their names.'<sup>280</sup> Not even Collins could help him: with *Recorde* and Digges, Wallis had more or less exhausted the sixteenth-century English contribution to algebra. Robert Norman was an instrument maker and his *Safe-guard for sailors*, on navigation, was in the Savile Library but he was not by any stretch of the imagination an algebraist.<sup>281</sup> Wallis's knowledge of *Recorde*'s work was unexpectedly scanty: he could not even name *The whetstone of witte*. Perhaps Collins gave him some guidance here, for in his *Additions and emendations*

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<sup>273</sup> Bombelli's *L'algebra* is not listed in Bernard 1697 and has not been added to the Savile collection since.

<sup>274</sup> Ramus 1560 [Savile Y.31, edition of 1586].

<sup>275</sup> Salignac 1580 [Savile V.22].

<sup>276</sup> Stevin 1585 [Savile Q.10].

<sup>277</sup> Clavius 1608 [Savile Bb.7].

<sup>278</sup> Henisch 1605.

<sup>279</sup> *Treatise of algebra*, 63.

<sup>280</sup> Rigaud 1841, II, 607.

<sup>281</sup> Norman 1584.

written while *A treatise of algebra* was in press Wallis was able to add the title and date of *The whetstone*. He also alluded to *The grounde of artes* (1543) and *The pathway to knowledge* (1551) but seemed to think they were the same book under different titles. He later acquired a copy of *The whetstone* for it is now in the Savile Library, with IOHN WALLIS on the spine and Wallis's annotations inside.<sup>282</sup> Wallis even managed to find one other, rather later, English writer, the little known John Tapp, whose book entitled, like Recorde's, *The pathway to knowledge*,<sup>283</sup> was published in 1613 but was essentially a revised translation of an earlier Dutch text, Nicolaus Petri's *Practique om te leeren reekenen*, first published in Amsterdam in 1583.

All the cossist texts, Wallis observed, used language and notation which showed them to be rooted in Arabic texts predating the rediscovery of Diophantus. Viète, on the other hand, dropped the Arabic nomenclature and adopted instead that of Diophantus. Wallis made much of this, as he did of Viète's notation which, Wallis claimed, made possible a fully generalised or 'specious' arithmetic. Wallis suggested that Viète (trained in law) used 'species' in the legal sense in which a single named person may be taken as representative of any person in the same position.<sup>284</sup> Not that Viète, remarked Wallis, was the first to introduce the letters A, B, C into algebra. Buteo had already done so in his *Logistice* of 1559 where he had solved the set of simultaneous equations:

$$1A + \frac{1}{3}B + \frac{1}{3}C = 14$$

$$1B + \frac{1}{4}A + \frac{1}{4}C = 8$$

$$1C + \frac{1}{5}A + \frac{1}{5}B = 8$$

(In fact the use of A as a second unknown, in addition to the usual cossist R, went back even further, to Stifel, who introduced it in his *Arithmetica integra* in 1543, and even used AAA to represent the cube of A. Since it was rare for cossist writers to venture beyond a single unknown, however, the potential of this additional notation remained unexplored.)

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<sup>282</sup> Recorde 1557 [Savile H.12].

<sup>283</sup> Tapp 1613.

<sup>284</sup> Not only his legal training but his activities as a cryptanalyst may have influenced Viète's perception of algebra, see Pesic 1997.

Wallis completely overlooked Viète's most important innovation, the application of algebra to Classical geometry, and therefore also ignored Viète's description of algebra as the 'analytic art'. Instead, Wallis referred to Viète's algebra as 'specious arithmetick', a name which emphasised the older arithmetic foundation of Viète's work. There is no clearer example of the different preconceptions attached to the various descriptions of algebra.

Wallis saw correctly that many of Viète's precepts were old rules in new notation, but conceded that Viète had added 'many new Inventions of his own, for the better understanding the Reasons of those Rules, and the more convenient management of them, with many great improvements thereof'.<sup>285</sup> What these inventions and improvements were he declined to say; the reader could find them for himself in Viète's work, now easily available, or in the work of later authors. In particular, said Wallis, William Oughtred 'hath contracted much of it into less room'.<sup>286</sup> This was Wallis's cue for introducing Oughtred and the seventeenth-century English mathematicians: from this point on, Wallis's history of algebra was to be a history of English algebra alone.

## Conclusion

Wallis's account of medieval and Renaissance algebra is lacking in several important respects, and its historical interest now is as a window into Wallis's perceptions rather than into the period it purports to describe. Wallis's main preoccupation was with listing sources rather than analysing content, and for the period before printed books he had little to go on: the Arabic origins of algebra and Leonardo's *Liber abbaci* had become part of mathematical folklore but Wallis knew both only from hearsay, and nothing of algebraic developments elsewhere in Europe before 1494. For the sixteenth century he had plenty of sources but failed to discriminate in any serious way between them, listing authors and dates without any reference to provenance (unless it was England) and without any analysis of individual contributions. Taking Cardano as just one more name in a list of foreign writers, Wallis missed the crucial innovative steps on which so many future developments were based,

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<sup>285</sup> *Treatise of algebra*, 66.

<sup>286</sup> *Treatise of algebra*, 66.

and as a result he was unable to contextualise (had he wanted to do such a thing) the work of Bombelli or Viète, or later, Harriot or Descartes. His neglect of the *Ars magna* renders his account of Renaissance algebra almost meaningless.

Wallis's most important contribution to a discussion of sixteenth century algebra came not in his thirteenth and fourteenth chapters but, perhaps inadvertently, in his first, where he gave the various names by which algebra had come to be known by about 1600. Although Wallis did not develop the theme, the different names in fact correspond to separate historical trends in the evolution of algebra and thus provide a useful framework for understanding some of the distinct aspects of early European algebra, a framework which has been followed in this chapter.

Wallis's superficial treatment of algebra in the period 1150-1600 is in strange contrast to the care with which he had earlier researched the spread of the numeral system. The clue to his approach is perhaps to be found in his final sentence and his introduction of Oughtred: there it becomes clear that his aim was not so much to explore Renaissance algebra for its own sake but to set the scene for an exposition of English writers, to create a backdrop against which the achievements of his compatriots would stand out to best advantage. By the end of his fourteenth chapter Wallis probably felt he dealt thoroughly enough with centuries past and could turn to what he knew best, the characters and events of his own lifetime.





## Chapter 4

### Ariadne's thread : the life and times of Oughtred's *Clavis*

#### Summary

William Oughtred's *Clavis mathematicae*, first published in 1631, was regarded for the remainder of the seventeenth century as a classic text in algebra, reprinted, translated and explained for over seventy years. Yet its content was limited, its style obscure and its notation old-fashioned even when it was first written, and it now seems extraordinary that it should have had such a long life. The early success of the book, and the great esteem in which Oughtred was held by contemporary and later mathematicians, can only be understood in the light of the general state of algebra teaching in England during the first half of the seventeenth century. In later years the book was kept alive by the efforts of one of its greatest admirers, John Wallis. This chapter sets the story of the *Clavis* into the wider context of English algebra, and the political background of the time.

William Oughtred was born at Eton in 1573.<sup>287</sup> A contemporary source claimed that Oughtred's father, Reverend Benjamin Oughtred, was the 'pantler', or butler, at Eton college,<sup>288</sup> but his true position appears to have been that of registrar.<sup>289</sup> He was also said to have taught writing and to have understood 'common arithmetique', and so would

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<sup>287</sup> The year of Oughtred's birth is here inferred from his portrait in Oughtred 1647, which gives his age as 73 in 1646. Aubrey in Aubrey 6, ff. 39-40, gave his date of birth as 5 March 1574 (or 1575 in the Julian calendar) but also said that Oughtred was 88 when he died in June 1660, implying a date of birth in March 1572 (English) or 1573 (Julian). Aubrey's inconsistency has been reflected in dates given by later commentators: Scott 1974 gave 1575; Cajori, 1916 gave 1573-1575.

<sup>288</sup> Aubrey 1898, II, 106-114.

<sup>289</sup> Willmoth 1993, 44.

probably have been responsible both for Oughtred's neat and beautifully formed italic hand, and his early interest in mathematics. Oughtred was later educated as a King's scholar at Eton and then from the age of fifteen at King's College, Cambridge, where he wrote his first mathematical treatise, *A most easie way for the delineation of plaine sun-dials*.<sup>290</sup> After graduating MA in 1600 he was ordained in 1603, and by 1610 was rector of Albury, just below the ridge of the North Downs near Guildford in Surrey, where he was to remain until his death fifty years later. During his early years at Albury, Oughtred kept his parish registers meticulously and travelled no further than an occasional visit to London, but he gained a reputation for his skill in mathematics. Already in 1616 John Hales, formerly an Oxford lecturer in Greek and now fellow of Eton, consulted Oughtred on mathematics and wrote:<sup>291</sup>

Either your facility was great or your pains very much, who could in so short a space discharge yourself of so many queries . . . Amongst all the solutions which you then sent me, none there was which gave me not full and sufficient satisfaction, (and so I persuade myself would have given to one of deeper skill than myself,) one only excepted, and that is concerning the projecture of an oblique circle.

Oughtred became known to Sir Charles Cavendish (1591-1654), who was the brother of the Duke of Newcastle and noted for his own ability in mathematics. The connection may have been made through Thomas Howard, Earl of Arundel and Surrey, a well known patron of the arts who also took a gentlemanly interest in mathematics and whose country home at West Horsley was about five miles from Albury. Cavendish, from about 1617 onwards, devoted himself to collecting mathematical books and manuscripts, and cultivated contacts with several eminent continental mathematicians, including Descartes. Through him, Oughtred would have become familiar before most other English mathematicians with Viète's 'analytic art', the application of analytic and algebraic methods to

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<sup>290</sup> Oughtred 1596.

<sup>291</sup> Rigaud 1841, I, 3.

Classical geometry.<sup>292</sup> I have recently discovered Oughtred's margin notes to Cavendish's copy of Viète's *De aequationum recognitione* (1615), carefully copied out by John Pell,<sup>293</sup> the only evidence so far known of Oughtred's mathematical reading in his mature years.

In 1628 Oughtred was engaged as tutor to Howard's fourteen year old son, William; he had earlier been employed in a similar capacity by another local notable, Sir Francis Aungier, later Baron Longford. Oughtred was already over fifty years old, and had not written anything mathematical since his Cambridge days, but encouraged by Cavendish (who would have known that Thomas Harriot's work was also being prepared for publication) he wrote his first textbook, on arithmetic and algebra, in 1630. It was published early the following year under the title *Arithmeticae in numeris et speciebus institutio: quae tum logisticae, tum analyticae, atque adeo totius mathematicae quasi clavis est* ('The method of calculating in numbers and letters: which was the key first to arithmetic, then to analysis and now to the whole of mathematics'). It was dedicated to the young Lord William Howard, to his parents and to Cavendish. The running title head of the first edition was *Clavis mathematicae*, and the book became commonly known as the *Clavis*, the title used in this thesis.

### English algebra texts before 1630

The *Clavis* was written at a time when few good algebra texts were available to English readers. In 1596 William Phillip had made an English translation of Nicolaus Petri's *Practique om te leeren reekenen* first published in Amsterdam in 1583. A literal translation of the Dutch title is *The practice of learning calculation*, but Phillip called his work *The pathway to knowledge* (not to be confused with Recorde's 1551 work of the same name which dealt purely with elementary geometry). From Wallis's later account it would seem that Phillip translated the text as loosely as the title, either mistranslating technical terms or

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<sup>292</sup> Harriot also knew Viète's work but there was no known connection between Oughtred and Harriot, though Oughtred may have known of Harriot's work through Cavendish.

<sup>293</sup> British Library Add MS 4423, ff. 146-153<sup>v</sup>.

simply leaving them in Dutch.<sup>294</sup> A revised version of Phillip's book was the single algebra text to be published in England during the entire first thirty years of the seventeenth century (see Table 3). The new version was again called *The pathway to knowledge*, and the author was John Tapp, better known as a writer on navigation.<sup>295</sup> The preface advised:

Know that the ground and original of this work was the book formerly known by this title (namely *The Pathway to Knowledge*) which being as I take it translated out of Dutch . . . was published in many places not only obscure and difficult for a learner's apprehension, but also somewhat confused as having both rules and questions of cosse and vulgar arithmetic mixed defusedly one with another . . .

To Phillip's weak translation Tapp added from other authors:

. . . for finding the nearest root a number not a right square or cubicall I have observed the method used in Gosselin upon Tartaglia, and for the rules and questions in the Arte of cossicke numbers it is only a literal translation collected out of Mr Valentine Mennher his Arithmeticke.

Mennher's *Arithmetique seconde* had first been published in Antwerp in 1556, and Gosselin's edition of Tartaglia's *L'arithmetique* in Paris in 1578, so Tapp's book, by his own admission, was little more than a compilation of much earlier texts in French or Dutch, written between thirty and sixty years before his own. This single small volume speaks clearly enough of the low general knowledge of algebra in England in these years. The work of Harriot was as yet unpublished and unknown.

Had Oughtred looked to foreign text books for his young pupil, the situation was hardly better; the majority of basic algebra texts published anywhere in Europe during this period were no more than new editions of much older sixteenth-century works, and of the writers, predominantly German, who published new books between 1600 and 1630, about two thirds were now writing

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<sup>294</sup> Wallis 1685a, 157.

<sup>295</sup> Tapp 1613; Tapp 1596. Both books were based on translations from foreign texts.

in their own languages (see Table 2). The books still appearing in Latin from about 1610 onwards began to show a marked decline in clarity of expression or understanding. Thus Ioane Lantz, in his *Institutiones arithmeticae* of 1616 used the old cossist symbols R, Z and C for ‘things’, squares and cubes, and multiplied  $3R$  by  $4A$  to give  $12RA$ , but found the cube root of  $27AC$  to be  $3A$ , so introducing ambiguity as to whether C was the cube of a particular quantity or the general symbol for a third power. Francis Brasser’s *Arithmetica* of 1619 was muddled and old-fashioned in its appearance. The *Algebra* of Hermannus Follinus of 1622 followed the style of the cossist texts but did not even go as far as defining quadratic equations, stating instead that the square root of  $18R-72$  was 12, betraying a sorry lack of understanding of the equation  $RR=18R-12$ . Works such as the *Algebra* of Christopher Clavius (published in 1608 but originally written in 1580) or those of Cyriaque de Mangin (1623)<sup>296</sup> remained useable texts but went no further than the cossist texts of the previous century. Oughtred’s work can therefore be seen as an important attempt to modernise algebra and algebraic notation.

### The 1631 *Clavis*

The *Clavis* was both small and concise: an octavo (17 cm x 11 cm) volume of twenty short chapters in just 88 pages, a feature which must have added to both its availability and its usefulness. Two of the six copies in the Bodleian Library, Oxford, are ‘softback’ versions covered in vellum, and no larger than a very slim modern paperback.<sup>297</sup>

The introduction to the first edition, written on 1 January 1631, began with remarks addressed to Lord William Howard:

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<sup>296</sup> Cyriaque de Mangin has confused posterity by writing under two pseudonyms, D. Henrion and Pierre Hérigone, as noted in the entry for Cyriaque de Mangin in the *Catalogue générale* 1929. The *Catalogue* lists separately the works written under each name. The books on algebra are all listed under Cyriaque de Mangin, in Rider 1982. The *Dictionary of scientific biography*, however, has separate entries for Henrion and Hérigone, thus implying that they were not the same person, even though some book titles are common to both bibliographies.

<sup>297</sup> Savile Z.19 and Savile Z.24

*Cum tibi, illustrissimi, tui patris jussu, in disciplinis Mathematicis exponendis deservierim, . .*

Most illustrious youth, since the time I have served you so devotedly, by order of your father, to expound the teaching of mathematics, I have wished nothing more, than that I might, in the best of faith, show this, that is, the Analytic method (of which teaching it is certainly part). And for this reason, I have recalled the demonstrations of Euclid, among other things, in the Analytic form, of which in the 19 chapters of this book, several examples are to be found.

The reference to nineteen chapters indicates that the twentieth chapter (of assorted problems) was added after the preface was written, in which case the ‘demonstrations of Euclid’ in the nineteenth chapter were the culmination of the original text. Oughtred clearly understood and endorsed Viète’s ‘analytic method’ as a way of re-writing Classical theorems in clearer, algebraic form, but beyond this he, like Viète before him, saw it also as a means of recovering the true meaning, and even the methods, by which such theorems were originally found. A few lines later he wrote:

Then I want to extend to students of mathematics, as it were, Ariadne’s thread, by the help of which they may be led to the innermost secrets of this knowledge, and directed towards an easier and deeper understanding of the most ancient and favoured authors.

Oughtred’s reference to Ariadne was itself Classical: Ariadne was the Cretan princess who gave her lover, Theseus, the thread by which he was able to find his way out of the Minoan labyrinth. Oughtred’s purpose was to use the method of analysis to understand and recreate the work of ancient writers, so that Ariadne’s thread was to be a guide not to the future but to the past. Oughtred described how the investigation of ancient writings was to be done by interpretation, comparison and reduction of equations, and in symbols which rendered these matters ‘clearer to the eyes’; the work was short, and rightly so, for he had not written for the ‘half-asleep’ but for those who preferred their mathematics concise and brief. His method of teaching, he stated, was by problems and examples, and so that nothing

would be lacking from his text he had added the rules of arithmetic, and instruction in decimals, now more useful than the older sexagesimal system.

In the early chapters the *Clavis* differed little in content from its predecessors. Cossist texts were usually divided into two parts, which treated arithmetic first in numbers and then in letters, or 'species', (hence the common appearance of titles such as *Arithmeticae libri duo*); Oughtred changed this and dealt with numbers and letters side by side, but covered much the same content: the four operations of addition, subtraction, multiplication and division, followed by proportion and greatest common measure, and the difficult subject of fractions (in three chapters).

Oughtred followed Viète in using A and E for unknown quantities, but in Chapter 11 he introduced new notation by defining for two quantities A and E, the sum Z, difference X, product P and quotient R/S, and he explained how, given A or E and any one of Z, X, P or R/S, the others could be found by use of easy formulae. The text continued with a table of powers of numbers up to  $9^8$  and another for powers of (A+E), using an abbreviated form of Viète's notation:  $A_q$ ,  $A_c$ ,  $A_{qq}$  etc. The tables were put to use for finding square and cube roots, and Oughtred then dealt with surds in the usual way.

In Chapter 16 Oughtred reached the treatment of equations, and once again followed the pattern of the cossist texts in explaining the *Rule of Algebra* and the rules for handling equations.<sup>298</sup> Having dealt with these standard operations he moved on in the next two chapters to more difficult examples based on the preceding work. First he returned to methods of finding A and E from the four quantities Z, X, P, R/S: the relationship  $Z = (A_q + P)/A$  leads to a quadratic equation in A, and in the eighteenth and last of his theoretical chapters, Oughtred extended his notation to include  $\mathcal{A}\mathcal{E}$  for AE and  $\mathcal{Z}$ ,  $\mathcal{X}$ ,  $\mathcal{Z}$ ,  $\mathcal{X}$  for sums and differences of squares and cubes:

$$\mathcal{Z} = A_q + E_q$$

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<sup>298</sup> Mahoney 1994, 27 n3, claims that Chapter XVI of the *Clavis* was taken directly from Chapter V of Viète 1591. Although Oughtred adopted Viète's notation he never took up his vocabulary, and in fact both chapters were actually based on the contents of many earlier texts.

$$X_q = A_q - E_q$$

$$Z = A_c + E_c$$

$$X = A_c - E_c$$

and then gave a long list of identities written in this notation, for example:

$$3. A_c + 3AE_q - 3A_qE - E_c \text{ est } C:A-E \text{ [or } (A-E)^3]$$

$$6. Q:A + \frac{1}{2}E \text{ [or } (A + \frac{1}{2}E)^2] = Z + \frac{1}{2}E - \frac{3}{4}E_q$$

$$8. Z_q - X_q = 4\frac{1}{2}E_c$$

All this was put to use in the two final chapters. In Chapter 19, Oughtred at last gave the results from Euclid promised in his preface, writing each of the fourteen propositions of Book II in purely algebraic form and without diagrams, an entirely new departure in an elementary text. (Harriot had transcribed these propositions from Euclid II into algebra many years before but never published them; Descartes' algebraic geometry was not published until 1637.<sup>299</sup>) Propositions II.5 and II.6 led to quadratic equations, expressed in Oughtred's notation as:

$$ZA - A_q = \frac{1}{2}E$$

$$A_q + ZA = \frac{1}{2}E$$

$$A_q - ZA = \frac{1}{2}E$$

Oughtred introduced the symbol  $\pm$  and gave, again for the first time in such a text, the solution formulae (though he never gave negative or imaginary roots):

$$A = \frac{1}{2}Z \pm \sqrt{\frac{1}{4}Z_q - \frac{1}{2}E}$$

$$A = \sqrt{\frac{1}{4}Z_q + \frac{1}{2}E} \pm \frac{1}{2}Z$$

Chapter 20 went on to give twenty further examples of the application of algebra to geometry. The most interesting was the final one in which Oughtred derived algebraic formulae for the trisection and quinquisection of angles; he was unable to give geometric constructions but hoped that with the help of his new methods, solutions to this ancient problem might yet be discovered.

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<sup>299</sup> British Library Add MS 6785, ff. 153-156.



### The contribution and influence of the first edition

Much of the content of the *Clavis* was already well known and had been treated many times before. What did Oughtred add that was new? Charles Hutton, in his article on algebra in *A mathematical and philosophical dictionary* in 1796, carefully assessed the innovations of each new text, and noted how much of Oughtred's material derived from Viète, but credited Oughtred with the introduction of 'various symbolical marks and abbreviations which are not now used', 'the first instance of applying algebra to geometry', and 'a good tract on angular sections'.<sup>300</sup> Hutton's remark on notation was substantially correct; Oughtred's use of  $\times$  for multiplication was masterly but his other innovations,  $\mathbf{Z}$ ,  $\mathbf{X}$  and so on, were as incapable of generalisation as the cossist symbols which preceded them, and no more than temporarily helpful. Oughtred's notation enabled him to write identities in a variety of ways, but did nothing to reveal the more general relationships which had to be understood in order to move from the known to the unknown, a limitation which could in the end only hinder rather than help the development of true algebraic thinking. On the second point, Hutton was plainly wrong: Viète's programme of recovering geometric theorems through algebra had been under way for forty years and was familiar to Oughtred who, as his preface plainly showed, was also using algebra for the purpose of understanding the works of Classical writers. Where Oughtred led the way was not in attempting such problems but in introducing them for the first time into an elementary text book. The unique achievement of the *Clavis* was that in one volume it combined the basic rules and methods of cossist algebra with applications of algebra to geometry. In this way it filled, as no other book then did, the vacuum between elementary texts of poor quality or in foreign languages, and the more sophisticated and not easily available works of Viète and his followers.

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<sup>300</sup> Hutton 1796, 91-92; Hutton 1812, 286-288. The sections on Oughtred are the same in both articles.

Had good textbooks been more common, the *Clavis* might have appeared unremarkable; instead it became a classic, and made Oughtred's name. Two years after publication William Robinson wrote to Oughtred (already using the abbreviated form of the title):<sup>301</sup>

I shall long exceedingly till I see your *Clavis* turned into a pick-lock; and I beseech you enlarge it, and explain it what you can, for we shall not need to fear either tautology of superfluity; you are naturally concise and your clear judgement makes you both methodical and pithy.

By 1635 Franc Derand was writing to Cavendish that the *Clavis* was 'in great estimation amongst the mathematicians at Paris',<sup>302</sup> though this may have been mere flattery on Derand's part to a known admirer of Oughtred, for by now the French had Albert Girard's *Invention nouvelle en l'algebre*, a much clearer text than the *Clavis*. In England, however, the *Clavis* had no competitor. In 1636 Robinson wrote again, and his letter implies that an English translation was already under way. He also suggested that for some readers the compactness of the book presented difficulties, and begged Oughtred to expand his teaching.<sup>303</sup>

I will once again earnestly entreat you, that you be rather diffuse in the setting forth of your English mathematical *Clavis*, than concise, considering that the wisest of men noted of old, and said *stultorum infinitus est numerus* [the stupid are infinite in number], these arts cannot be made too easy, they are so abstruse of themselves . . . Brevity may well argue a learned author, that without any excess or redundance, either of matter or words, can give the very substance and essence of the thing treated of; but it seldom makes a learned scholar; and if one be capable twenty are not;

Among those who sought personal instruction from Oughtred were Seth Ward and Charles Scarborough, who in 1642 were both young Fellows at Cambridge. According to Aubrey, they 'came [to Oughtred] as in Pilgrimage, to see him and

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<sup>301</sup> Robinson to Oughtred 11 June, probably 1633, Rigaud 1841, I, 16.

<sup>302</sup> Derand to Cavendish 11 February 1635, Rigaud 1841, I, 23.

<sup>303</sup> Robinson to Oughtred 2 July 1636, Rigaud 1841, I, 26.

admire him' and 'to be enformed by him in his *Clavis mathematicae*, which was then a booke of *Aenigmata*'.<sup>304</sup> Ward and Scarborough afterwards taught their students from the *Clavis*, and so raised it to the status of a university textbook, though their direct influence was short lived as teaching at Cambridge fell into disarray on the outbreak of the Civil War, and both Ward and Scarborough were forced to leave.

Oughtred also received written requests for help and William Price's letter of 1642 was probably only one of several:<sup>305</sup>

Sir,

Though I am a stranger to your person, yet I am well acquainted with the fame of your singular skill in the mathematics, and thereupon have so far presumed, to intreat your assistance for the geometrical solution of the inclosed diagram, which, to you that have attained the perfection of the analytical art, perhaps will not appear difficult.

Oughtred's reply indicated that his renown in mathematics had brought other problems in its train and that now, nearing seventy, he was becoming reluctant to take on new work, perhaps explaining why the English translation expected by Robinson in 1636 had never appeared:<sup>306</sup>

But now being in years and mindful of mine end, and having paid dearly for my former delights both in my health and state, besides the prejudice of such, who not considering what incessant labour may produce, reckon so much wanting unto me in my proper calling, as they think I have acquired in other sciences; by which opinion (not of the vulgar only) I have suffered both disrespect, and also hinderance in some small preferments I have aimed at. I have therefore now learned to spare myself, and am not willing to descend again *in arenam*, and to serve such ungrateful muses. Yet, sir, at your request I have perused your problem, . .

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<sup>304</sup> Aubrey 1898, II, 108, 284.

<sup>305</sup> Price to Oughtred 2 June 1642, Rigaud 1841, I, 59.

<sup>306</sup> Oughtred to Price 6 June 1642, Rigaud 1841, I, 60-61.

That complaints were indeed made against Oughtred was borne out by Aubrey: 'I have heard his neighbour ministers say that he was a pittiful preacher; the reason was because he never studyed it, but bent all his thoughts on the mathematiques'; however, 'when he was in danger of being sequestered for a royalist, he fell to the study of divinity, and preacht (they sayd) admirably well, even in his old age.'<sup>307</sup>

The Civil War was the most difficult time of Oughtred's life, and the danger of sequestration, according to Aubrey, prompted Oughtred to more than just renewed zeal in preaching:<sup>308</sup>

Notwithstanding all that has been sayd of this excellent man, he was in danger to have been sequestered, and . . . Onslowe that was a great stickler against the royalists and a member of the House of Commons and living not far from him - he translated his *Clavis* into English and dedicated it to him to clawe with him, and it did soe his businesse and saved him from sequestration. Now this Onslowe was no scholar and hated by the country [county] for bringing his countrymen of Surrey into the trap of slaughter when so many petitioners were killed at Westminster and on the roads in pursuite, anno Domini 16- -.

Oughtred's own account of how the second edition of his book came to be published was a little different:<sup>309</sup>

I was unwillingly drawne, at this my declined age, to appear unto the world in such a kinde of Subject. But occasion was administred by one Mr Seth Ward, a young man excellently accomplished with all parts of polite Literature, then Fellow of Sidney Colledge in Cambridge, who tooke the pains to seek me out at my house, and by a gentle violence induced me to publish again my former Tractate in a manner new moulded and perfected: And also divers pieces (among many) which I had long agoe commented and digested, some compleatly, and some more rudely, without any intent to make the same publick.

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<sup>307</sup> Aubrey 1898, II, 111.

<sup>308</sup> Aubrey 1898, II, 110.

<sup>309</sup> Oughtred 1647, Preface.

There was probably truth in both accounts. ‘Onslowe [who] was no scholar’, to whom the English translation of the *Clavis* was flatteringly dedicated, was Sir Richard Onslow, Member for Surrey in the House of Commons for 1627-28, who had raised his own regiment for the Parliamentary side in 1642 and was appointed one of the sequestrators for Surrey in 1643. Oughtred’s successful appeal against sequestration was probably heard in 1646, and the English version of the *Clavis* was prepared for publication that same year, with a portrait of Oughtred in suitably puritan dress.<sup>310</sup> Even if the immediate danger had passed, it might still have seemed expedient or grateful to stay on the right side of Onslow, and Ward, who had spent time with Oughtred after his own expulsion from Cambridge and who became astute in bending before the political wind,<sup>311</sup> possibly had just such a motive in mind when he persuaded Oughtred to republish.

#### **The second editions: 1647 and 1648**

The first English translation of the *Clavis* was done by Robert Wood (1622-1685), then a student in his early twenties at Merton College, Oxford. It bore the title *The key of the mathematics new filed*, and was published in 1647 with a preface in which Oughtred answered those who complained that his work was difficult, and showed his own clear understanding of the advantages of the new algebra:

Which treatise being not written in the usuall syntheticall manner, nor with verbous expressions, but in the inventive way of Analitice, and with symboles or notes of things instead of words, seemed unto many very hard; though indeed it was but their owne diffidence, being scared by the newnesse of the delivery; and not any difficulty in the thing it selfe. For this specious and symbolicall manner, neither racketh the memory with multiplicity of words, nor chargeth the phantasie with comparing and laying things together; but plainly

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<sup>310</sup> For a fuller account of the sequestration proceedings against Oughtred, see Willmoth 1993, 56-60.

<sup>311</sup> Ward was deprived of his Cambridge Fellowship for writing against the Covenant in 1644. He later swore the Oath of Allegiance to the Commonwealth when offered a professorship at Oxford in 1648 but never committed himself entirely to the new regime and was eventually rewarded with a bishopric after the Restoration.

presenteth to the eye the whole course and process of every operation and argumentation.

And, in a mixture of metaphors, he restated the purpose of his book:

Now my scope and intent in the first Edition of that my Key was, and in this New Filing, or rather forging of it, is, to reach out to the ingenious lovers of these Sciences, as it were Ariadne's thread, to guide them through the intricate Labyrinth of these studies, and to direct them for the more easie and full understanding of the best and antientest Authors.

A parallel Latin edition was published the following year, identical in content to the 1647 English version but without the dedication or preface, and with two extra appendices: one a transcription of Euclid X into symbols, the other on regular solids (see Table 4).

Oughtred chose his words aptly when he described this second edition as a new forging, for there were significant changes from the first edition, many perhaps suggested by Ward or Wood. The early chapters carried a number of minor additions, with longer expositions of sexagesimal arithmetic, and of arithmetic, geometric and harmonic progressions added to the chapter on proportion. In Chapter 11 Oughtred defined the more advanced notations  $\mathbb{Z}$ ,  $\mathbb{X}$ ,  $\mathbb{Z}$ ,  $\mathbb{X}$ , previously introduced towards the end of the book. Chapters 12 to 15 were unchanged, but Chapter 16, on the formation and reduction of equations, was much expanded with a section dealing specifically with quadratic equations, another on the application of this work to the squares and roots of binomials (quantities of the form  $a \pm \sqrt{b}$ ), and some preliminary work on angular sections. Chapter 17 had disappeared (its most significant content now in Chapter 11) and Chapter 18 was also reduced, its contents transferred to the next chapter.

The most significant extension of the book was to the original Chapter 19 (now renumbered 18) on Euclid II, which was almost completely re-written and entitled 'The analitical store'.<sup>312</sup> Propositions II.5-II.10 remained, but to these Oughtred added a collection of what he called 'analyticall furniture': identities on

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<sup>312</sup> The same metaphor was later taken up by Newton: see Newton 1967-81, II, 393.

squares and cubes from the original Chapter 18 (but not cubic equations); useful formulae for circles, cylinders and spheres; and twenty theorems and nineteen constructions (from Euclid I, III and VI) which the intending analyst should know. The last four propositions II.11-II.14 of Euclid II (today known as the 'golden section' and the 'cosine rule') were moved to the final chapter, where they were followed by a new section on arithmetic progressions, and the twenty problems of the original twentieth chapter. The new edition also carried a long appendix on the numerical solution of equations, short pieces on the calculation of interest and the rule of false position, and Oughtred's early treatise on sundials.

The result of these changes was not only a longer text (120 pages in English, 112 in Latin), but one which had lost the methodical structure of the original. Elementary as it was, the 1631 version led the reader by a recognisable and well worn route, and introduced new ideas in such a way as to build steadily towards the more difficult final chapters. In the new version the reader came suddenly upon new and strange notation at Chapter 11, which then disappeared until put to use in the difficult new material added to Chapter 16. The final chapters were no longer a gentle introduction to algebraic geometry but an encyclopaedia of miscellaneous results and problems. If the changes improved the usefulness of the *Clavis* as a reference book, they certainly did not improve its structure. However, from this point on the text was essentially established and was to survive in future editions without any further significant alteration.

### **The third edition: 1652**

In 1652 the *Clavis* was published in its third Latin edition, this time at Oxford. Why was a further edition thought to be necessary so soon? A letter from Oughtred to Ward concerning this Oxford edition has survived among Aubrey's papers, and in addition to the familiar names of Ward, Wood and Scarborough, it also mentions Wallis:<sup>313</sup>

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<sup>313</sup> MS Aubrey 6, f. 40; Aubrey 1898, II, 113-114.

Worthy Sir,

I made bold lately when I sent my book in a letter to Mr Wood to nominate you and Mr Wallis together with him, to whose judgement and discretion I commit all my right and interest for the printing thereof at Oxford. I have sent the Epistle [preface], which, though written long since, yett was soe mislayed and mingled with many other papers, that I thought it lost. Therin I make noe unloving mention of your self and Dr Scarbrough, whose surname [sic] I remember not. I hope neyther of you will take my officiousnesse in evell part. Yett yf anything shall displease, you are intreated of me to alter it or raze it with a blott; but yf in and by your suffrage it maye passe, I would intreat you to supplie the Doctor's surname . . So you will be pleased to remember my best respects to Mr Wallis and favourably to pardon this troublesome interruption of him who am

Your truly loving friend to my power

Willm Oughtred

Aldburie

April 19 1651

Wallis was now Savilian Professor of Geometry, while Ward had been appointed to the chair in Astronomy. The two men were the same age but Wallis had begun his mathematical studies very much later: the important influence of the *Clavis* on Wallis's mathematical development will be discussed in more detail later, but he almost certainly visited or corresponded with Oughtred during the late 1640s, and became known as one of his pupils.<sup>314</sup> Wallis and Ward therefore shared a personal admiration for Oughtred, and probably both saw the value of introducing the *Clavis* to Oxford (as Ward had previously done to Cambridge) in a new edition cleared of errors and misprints and under an Oxford imprint. The

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<sup>314</sup> Aubrey 1898, II, 108.



new edition was printed by Leonard Lichfield, one of the official printers to the University,<sup>315</sup> and according to a note in Pell's papers cost four shillings bound.<sup>316</sup>

The third edition carried a new preface which explained that the book was originally written for William Howard (now Viscount Stafford, and in exile), and repeated both the advantages of the analytic method, and the metaphor of Ariadne's thread. Then Oughtred described, as quoted above, how he was persuaded to bring out the second edition of the *Clavis* by Ward, supported by Scarborough. For removing typographical errors, checking the calculations and generally supervising this new third edition with 'unbroken assistance and persistent scrutiny' he thanked Wallis. There were warm thanks for Wood, and the final acknowledgement was to Christopher Wren, then a student at Wadham College 'from whom we may expect great things', and whose translation of Oughtred's treatise on sundials appeared at the end of the appendices.

Now nearly eighty, Oughtred must have been grateful for the help of these younger men, and rarely can any textbook have boasted such illustrious support. Oughtred's admirers were now reaching the prime of their careers. Ward and Wallis had their Oxford Professorships; Scarborough was a Fellow of the Royal College of Physicians, later to be personal physician to Charles II and James II; Jonas Moore, not mentioned in the 1652 preface but acknowledged in 1647, was tutor to the young Duke of York, the captive son of Charles I. These were the first of Oughtred's pupils to attain positions of eminence, but Wren, still only twenty, was representative of a new generation who were to learn their mathematics from the *Clavis*. Robert Boyle wrote to Samuel Hartlib in 1647 when he too was twenty years old:<sup>317</sup>

The Englishing of, and additions to Oughtred's *Clavis mathematicae* does much content me, having formerly spent much study on the original of that algebra, which I have long since esteemed a much more instructive way of logic, than that of Aristotle.

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<sup>315</sup> In 1652 there were two such printers, Leonard Lichfield and Henry Hall. See Madan 1908.

<sup>316</sup> British Library Add MS 4431, f. 109.

John Locke (born the same year as Wren) was to write as late as 1681 that ‘the best algebra yet extant is Oughtred’s’.<sup>318</sup> Isaac Newton, who first read the *Clavis* (in its 1652 edition) in 1664, and never met Oughtred, still spoke of him thirty years later as ‘a man whose judgement (if any man’s) may be relyed on’.<sup>319</sup> To this long list of seventeenth century luminaries may be added many others less well known who cut their mathematical teeth on the *Clavis*.

### Rivals to the *Clavis*

For nearly twenty years after the first publication of the *Clavis*, no alternative elementary textbook was published in England (see Table 3). Harriot’s *Praxis*, published in the same year as the *Clavis*, was not for beginners. In 1650, for the first time, two new texts appeared but neither was a serious rival to Oughtred’s. The first was Richard Balam’s *Algebra, or the doctrine of composing, inferring and resolving an equation*, a tiny volume, which can only have sown confusion and despair in the minds of its readers. Balam introduced strange new notation at will: Dp4 for -4, Dq4 for +4 and ‘:A:’ for the ‘triplicate or cube’ of A (though ‘triplicate’ and ‘cube’ are not the same), and later also A(3) for the cube. But the text was primarily verbal rather than symbolic, and if Balam’s notation was obscure, his vocabulary was even more so. A few short examples will suffice to give the flavour of this strange little book:

All the affirmed nomes in a multinomiall are addends and all the denied nomes are subducends, to be subducted from the sum of the Addends, and they are to be composed in one summe before subduction . .

Two like inequimultiplicats (or cossick proportionals) are as any two of their homologall factors . .

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<sup>317</sup> Boyle 1744, I, 24.

<sup>318</sup> King 1830, I, 227.

<sup>319</sup> Newton to Hawes 25 May 1694, no. 452 in Turnbull 1959-77, III, 364. A copy of the 1652 *Clavis* owned by Newton was in the Turner Collection secretly sold by Keele University in 1998.

The inferring of an equation, or equative inference, is a numeration, which from an equation given and precedent, inferres a consonant or new equation. That which I here call equative inference is not mentioned by this name, in any Algebraicall Writer, which I have seene. But surely this is the thing, of which are meant the 5 rules, which Mr Oughtred gives in the 16<sup>th</sup> chapter of his *Clavis*  
 ..

Surely too, anyone reading this would be only too pleased to return to Oughtred's text without seeking Richard Balam's help in the matter. However, the book must have had some success, for it was reprinted in 1653.<sup>320</sup>

A more serious text, also published in 1650, was Jonas Moore's *Arithmetick in two books*. Here too, Oughtred's influence was apparent, and not surprisingly since Moore had been one of his pupils. In the traditional way, the first of the 'two books' dealt with 'vulgar arithmetick' and the second with 'arithmetick in species', or algebra. The latter covered much the same ground as the first edition of the *Clavis* but in a more elementary way; Moore thought it necessary to point out, for instance, that in forming equations one was dealing only with numbers, not with the things to which they referred, such as men or money. The book included the solutions to quadratic equations expressed in Oughtred's notation, but elsewhere in the book Moore used the index forms  $a^2$ ,  $a^3$ , introduced by Descartes in 1637, which here appeared for the first time in an English printed text.

Amongst the general acclaim for the *Clavis*, there were just a few voices of dissent, as recorded by John Collins, who later wrote: 'I know many that did lightly esteem [Oughtred] when living, some whereof are at rest, as Mr. [Samuel] Foster and Mr. [Thomas] Gibson.'<sup>321</sup> Samuel Foster was Professor of Astronomy

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<sup>320</sup> A copy of the 1653 edition arrived at the Bodleian Library from an anonymous source in 1950.

<sup>321</sup> Collins to unknown recipient, undated, Rigaud 1841, II, 477. A tentative dating of this letter is possible from the reference to Collins' friend Dary who is described as a tobacco cutter. Until 1670 Dary was a gauger in Bristol and Newcastle (Rigaud 1841, II, 176, 198) but by 1673 was out of work (Rigaud 1841, II, 556) so Collins' letter may be supposed to date from the years after 1670. See also note 342.

at Gresham College from 1641 until his death in 1652. Little is known of Thomas Gibson, but a later letter from Collins to Gregory indicated that he died about 1657/8.<sup>322</sup> Gibson's *Syntaxis mathematica* was published in 1655 and was a radically new kind of textbook, explicitly indebted not to Oughtred but to Harriot and Descartes, as Gibson clearly stated in his preface to the reader:

The method here used is the same as in Master Harriot in some places, that is, in such equations as are proposed in numbers. And as in Des Cartes in some other places, that is, in such equations as are solid [cubic], and not in numbers.

Harriot's influence was immediately obvious in Gibson's notation:  $a, e$  for unknowns;  $b, c, d, f$  etc. for knowns, and generally  $aaa$  (but occasionally  $a^3$ ) for a cube. Gibson also used Harriot's  $<$  and  $>$  for inequality. As promised in the preface, quadratic and cubic equations were solved numerically using Harriot's method, and in the following theoretical section Gibson showed how an equation could be composed as a product of factors, and stated a number of general results: that an equation can have as many real roots as dimensions, not more, though sometimes fewer because of repeated roots; that the last [constant] term is the product of the roots (including negatives); that positive roots can be changed to negative by changing the sign of every even term of an equation; and Descartes' Rule of Signs for the number of positive roots. Finally he showed how to transform equations by appropriate changes of root, leading to the usual solution for cubic equations. A later section described the application of algebra to geometry; but Gibson made no attempt, as Oughtred did, to give geometrical constructions for his solutions, being content to leave his readers with algebraic solutions. All this was a world away from the cumbersome text and content of the *Clavis* and would have seemed to render the ideas of Harriot and Descartes accessible for the first time to the ordinary English reader, but Collins, generally well informed about both English and foreign texts, wrote in 1667 that he had

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<sup>322</sup> Collins to Gregory 25 March 1671, in Turnbull 1939, 180. In Rigaud 1841, II, 219, incorrect punctuation makes it appear that Gibson died in 1650. The sentence should read '... were lent to Gibson, deceased, in anno 1650...'.

never seen the book.<sup>323</sup> Gibson was evidently an obscure figure even in his own day, whose reputation was never likely to compare with Oughtred's, and after his death two years later neither he nor his book ever became widely known.

Samuel Foster appears to have been one of the strongest critics of the *Clavis*, for Collins later wrote that as long ago as 1649 'Mr Foster of Gresham College seldom heard it mentioned but took occasion to utter his dislike of it'.<sup>324</sup> Some of Foster's work was translated and published posthumously in 1659 as *Miscellanies or mathematical lucubrations* by John Twysden, who took the opportunity to add some work of his own, but Twysden was a lawyer and physician, not a mathematician and the work was of no great weight. Twysden was evidently ignorant of Foster's opinion of Oughtred's *Clavis* for in his acknowledgements to other authors he wrote:

Amongst them all let Mr William Oughtred, of Aeton, be named in the first place, a Person of venerable grey haire, and exemplary piety, who indeed exceeds all praise we can bestow upon him. Who by an easie method, and admirable Key, hath unlocked the hidden things of geometry.

By the time Oughtred died in 1660 the *Clavis* remained virtually unchallenged as the primary algebra text for aspiring mathematicians, and it was to outlive its author for many years yet.

#### **The fourth edition: 1667**

It was Wallis, always supportive of Oxford publishing, who suggested that a fourth edition of the *Clavis* should be printed following the disastrous loss of books during the Fire of London in 1666. (Ward was by now Bishop of Exeter and no longer actively engaged in mathematics.) By this time the Rahn-Pell *Algebra* was in the press, but the only other algebra published since the Foster-Twysden *Miscellanies* was Dary's *The general doctrine of equation* (1664), no more than a slim 16 page summary of the known rules for handling equations.

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<sup>323</sup> Collins to Wallis February 1668, Rigaud 1841, II, 484.

<sup>324</sup> Collins to Wallis February 1668, Rigaud 1841, II, 483.

The dearth of good alternative texts (apart from Gibson's neglected *Syntaxis*) remained.

After the death of Leonard Lichfield in 1657, ownership of the imprint of the *Clavis* had passed to his widow, Margaret. She happened to be a neighbour of Wallis, who suggested to Collins that Moses Pitts, a bookseller, should negotiate with her for the rights. Pitts had already expressed an interest in publishing some of Wallis's work,<sup>325</sup> but he was less enthusiastic about taking on the *Clavis*. Collins' reply to Wallis on 2 February 1667 gives an insight into the contemporary state of mathematical publishing.<sup>326</sup>

Reverend Sir,

I received yours, and communicated to Mr Pitts who very much honours your advice, and thanks you for it; but if there be any other that is willing to bargain for the said impression, he is not desirous to interpose for these reasons.

1. He is engaged in the Dr's [Pell's] book already: but chiefly, the impression is double the number that ought to have been of a mathematical book, the best whereof, though sure of sale, are but slow. Mr Brigg's *Arithmetica Logarithmica*,<sup>327</sup> being too numerous an impression, has been tendered about the streets at 1s 6d. each. The like I say of Mr Barrow's *Euclid*.<sup>328</sup> Mr Sutton and myself . . have bought divers of them at 1s a book in quires.

2. There are sundry tracts of Algebra expected; first from beyond the sea . .

The foreign books expected (some of them posthumous publications) were from Chaveau,<sup>329</sup> Hudde<sup>330</sup> Tacquet,<sup>331</sup> Renaldini,<sup>332</sup> Fermat<sup>333</sup> and Descartes,<sup>334</sup> and to

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<sup>325</sup> Collins to Wallis probably January 1667, Rigaud 1841, II, 468.

<sup>326</sup> Collins to Wallis 2 February 1667, Rigaud 1841, II, 470.

<sup>327</sup> Briggs 1624.

<sup>328</sup> Barrow 1655.

<sup>329</sup> Little is known about Chaveau, see Tannery 1895, and the expected publication never appeared. However, there is a manuscript copy of Chaveau's *Traicte d'algebre* among the surviving papers of Charles Cavendish, British Library MS Harl 6083, 350-379, and another among Pell's papers, British Library Add MS 4407, ff. 31-37<sup>v</sup>.

these Collins added the work of the Englishman, John Kersey,<sup>335</sup> now ready for the press. But Collins also recognised that the *Clavis* was no longer held in uncritical regard. He continued:

The said Mr Kersey hath made notes on the *Clavis* and to say the truth, doth not admire any thing in it, save what concerns the tenth and succeeding Elements of Euclid.

Mr Bunning, an aged minister, near Nuneaton in Warwickshire, hath commented on the *Clavis*, which he left with Mr Leybourne to be printed; but one Mr Anderson, a knowing weaver, told Mr Bunning that the *Clavis* itself, and his comment thereon, were immethodical, and the precepts for educing the roots of an adfected [polynomial] equation maim and insufficient.<sup>336</sup>

Despite these objections Collins reported that Pitts

. . is notwithstanding willing to deal for the impression, provided there be an engagement that it shall not be reprinted till the impression be sold; and because it is already common, that he may have liberty to increase it with such comments or explications as he shall be advised by his friends to be annexed to it;

Wallis replied three days later in somewhat defensive tone, that he had made the suggestion only in response to Pitts' expressed willingness to publish mathematical works, and in view of the convenience of using an impression already prepared. To the hesitations expressed by Pitts and Collins he replied:<sup>337</sup>

Whether the number be too great, or the book not so vendible, the bookseller, who understands his trade, is a more competent judge than I. But for the

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<sup>330</sup> Hudde, nothing published after 1659.

<sup>331</sup> Tacquet 1669.

<sup>332</sup> Renaldini 1669.

<sup>333</sup> Fermat 1679.

<sup>334</sup> Descartes, nothing published after 1659.

<sup>335</sup> Kersey 1673.

<sup>336</sup> Robert Anderson also disliked the work of Dary: see Anderson 1670. See also note 340.

<sup>337</sup> Wallis to Collins 5 February 1667, Rigaud 1841, II, 474.

goodness of the book in itself, it is that (I confess) which I look upon as a very good book, and which doth in as little room deliver as much of the fundamental and useful part of geometry (as well as of arithmetic and algebra) as any book I know; and why it should not be now acceptable I do not see. It is true, that as in other things so in mathematics, fashions will daily alter, and that which Mr Oughtred designed by great letters may be now by others be designed by small; but a mathematician will, with the same ease and advantage, understand  $A_c$ , and  $a^3$  or  $aaa$ . Nor will Euclid or Archimedes cease to be classic authors and in request, though some of their considerable propositions be, by Mr Oughtred and others, delivered now in a more advantageous way, according to men's present apprehensions. And the like I judge of Mr Oughtred's *Clavis*, which I look upon (as those pieces of Vieta who first went that way) as lasting books and classic authors in this kind;

Wallis thought Pitts' conditions reasonable though he himself was reluctant to see the book made larger:

For if Mr Oughtred had intended it to be large, he could with more ease have made it much bigger than it is. But it was by him intended, in a small epitome, to give the substance of what is by others delivered in larger volumes.

Finally, Wallis indicated that a price of £40, or 9½d per book, implying a print run of 1000 copies, would be acceptable to Mrs Lichfield.

The negotiations continued; Collins reported back to Wallis that Pitts was now working in partnership with another bookseller, Mr Thompson, in the matter and the pair would not offer more than £32.<sup>338</sup>

I cannot prevail with them to bid more for it; they say it is not a book so much inquired for here as in the universities, and they both doubt it will not sell without a comment; and Mr Thompson says he was long possessed of Mr Clarke's comment, who would freely have imparted it to any one to print, and presumes he may have it again if he request it, and affirms it is very large, and will make above 20 sheets.

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<sup>338</sup> Collins to Wallis undated, Rigaud 1841, II, 482.



The matter of the commentary was evidently resolved in Wallis's favour, for the *Clavis* was reprinted later the same year without any new additions, and not for Pitts and Thompson but for booksellers John Crosley and Amos Curteyne.

It seems that Bunning and Clark were not the only commentators on the *Clavis* about this time.<sup>339</sup> In 1671, Collins mentioned the late Dr (Richard) Rawlinson in this respect but held him in little regard:

One Isles, a bookseller, bought some of his books: and Anderson, a weaver, in company of Mr Streete, bought more of them;<sup>340</sup> and they have seen some of his writings, for which a great rate was demanded; and if I meet Streete accidentally, I shall with no great appetite inquire where they are.

Collins concluded that he was not keen to recommend any of the three commentaries, for he knew that Kersey's forthcoming book was better than any of them.

Gilbert Clark's commentary, mentioned more than once during the negotiations over the fourth edition of the *Clavis*,<sup>341</sup> finally appeared in 1682 under the title *Oughtredus explicatus, sive commentarius in clavem mathematicam Oughtredi* ('Oughtred explained, or a commentary on Oughtred's key to mathematics') but did no more than expand and explain the easier sections of Oughtred's original text. An important letter from Collins on the *Clavis* was probably addressed to Clark,<sup>342</sup> and is worth quoting at some length for the light it sheds on Collins' knowledge of earlier texts, and for his carefully considered appraisal of the value of *Clavis* by the early 1670s. Oughtred gave no indication of the sources of his work, and Collins began his letter by noting several continental authors who had published before him:

Worthy sir,

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<sup>339</sup> Collins to Vernon early 1671, Rigaud 1841, I, 151-154.

<sup>340</sup> Anderson and Streete together wrote a book on gunnery, Anderson and Streete 1674.

<sup>341</sup> Collins to Wallis 2 February 1667; Collins to Wallis February 1667, Rigaud 1841, II, 471, 483.

I have yours in answer to what was objected against the *Clavis*. It was not my intent to disparage the author, though I know many that did lightly esteem him when living, some whereof are at rest, as Mr Foster and Mr Gibson. I do not search *atramentum in nive* [blackness in the snow], but my design was to acquaint you with the argument of certain books, whereby the Author might be improved . . . Nor is the Author or any man blamed for making a collection of things already known. Collection, translation, and illustration of matters scarce, exotic, and obscure, cannot but have its encouragement. You grant the author is brief, and therefore obscure, and I say it is but a collection, which, if himself knew, he had done well to have quoted his authors, whereto the reader might have repaired. You do not like those words of Vieta in his theorems, *ex adjunctione plano, solidi, plus quadrato quadrati*, etc, and think Mr Oughtred the first that abridged those expressions by symbols; but I dissent, and tell you 'twas done before by Cataldus, Geysius, and Camillus Gloriosus, who in his first decade of exercises,<sup>343</sup> (not the first tract,) printed at Naples in 1627, which was four years before the first edition of the *Clavis*, proposeth this equation just as I here give it you, viz.  $1ccc+16qcc+41qqc-2340cc-18364qc-133000qq-5450c+3728q+8064N$  aequatur 4608, finds N or a root of it to be 24, and composeth the whole out of it for proof, just as in Mr Oughtred's symbols and method. Cataldus on Vieta came out fifteen years before,<sup>344</sup> and I cannot quote that, as not having it by me.

As for Geysius, he published an *Algebra and Stereometria*<sup>345</sup> divers years before the first edition of the *Clavis* was extant in Mr Harriot's method, out of which Alsted took what he published of algebra in his *Encyclopaedia*,<sup>346</sup> printed in 1630, the year before the *Clavis* was first extant (see Christmannus and

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<sup>342</sup> Collins to unknown recipient, undated, Rigaud 1841, II, 477-481. Rigaud mistakenly assumed the letter was to Wallis, but the style of address was not that customarily used by Collins to Wallis, who was also mentioned by name in the text. See also note 321 on the date of this letter.

<sup>343</sup> Gloriosus 1627-39.

<sup>344</sup> Cataldi 1622.

<sup>345</sup> Not traced.

<sup>346</sup> Alsted 1620.

Raymarus).<sup>347</sup> Mr Harriot's method is now more used than Oughtred's, and himself in the esteem of Dr Wallis not beneath Descartes . .

As for what Mr Oughtred hath done on the table of powers, I willingly suppose he had not seen Geysius or Faulhaber, whom Descartes visited, out of whose algebraic works hard copious matters may be taken.

Collins carefully refrained from blaming Oughtred for omitting the names of his predecessors: Collins wanted them mentioned only so that readers could return, if they wished, to the original sources. When it came to Oughtred's notation, however, his criticism was more direct:

And as for Mr Oughtred's method of symbols, this I say to it; it may be proper for you as a commentator to follow it, but divers I know, men of inferior rank that have good skill in algebra, that neither use nor approve it. One Anderson, a weaver, . . Mr Dary, the tobacco cutter, . . Wadley, a lighterman, . . and [I] may acquiesce in these men's judgments, or at least in Dr Pell's, who hath said it is unworthy to the present age to continue it, as rendering easy matters obscure. Is not  $A^5$  sooner writ than  $Aq$ ? Let  $A$  be 2. The cube of 2 is 8, which squared is 64: one of the questions between Maghet[,] Grisio<sup>348</sup> and Gloriosus is whether  $64 = \text{Acc}$  or  $Aq$ . The Cartesian method tells you it is  $A^6$ , and decides the doubt.

Collins continued:

As to the third objection, about the defect of argument, and fourth about the improvement of the general method, they cannot properly concern the author, nor is he to be blamed for not publishing what probably he knew not, which yet, in good part, was then extant in Gerrard<sup>349</sup> and Vieta *de Recognitione et Emendatione Aequationum*,<sup>350</sup> but those works of Vieta came out piecemeal, most of them at his own dispose, and thence became almost unknown and unprocurable.

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<sup>347</sup> Jacob Christmannus and Nicolaus Reimarus Ursus. It is not clear which of their writings are referred to here.

<sup>348</sup> Grisio 1641. A comma is missing between 'Maghet' and 'Grisio' in Rigaud 1841, II, 480.

<sup>349</sup> Girard 1629.

<sup>350</sup> Viète 1615.

Thanks to Pell's notes, we now know that Oughtred in fact read *De recognitione* with great care. Collins recognised, however, that Oughtred was writing within the limitations of his time, and what he now wanted was not that the *Clavis* should be abandoned, but revised and updated for modern readers:

The aim of those objections was not to disparage the author, but to incline you to supply the defect of him, that his book, together with yours, might be of the more durable esteem, and not be undervalued (as that author now is by Mr Hooke<sup>351</sup> and Dr Croone,) as wanting the most material parts of algebra.

I agree with you, the author is not to be rejected; he was, without doubt, a very learned divine and mathematician, and one that did much good in his generation. I know no man that would willingly be without his book, and certainly it had been a great detriment to learning to have wanted it.

Collins' reasonable and carefully stated opinion that the *Clavis* had been invaluable in its time, but was now capable of improvement, was only what any objective person might have argued about a book that had been in circulation for some forty years, and his view was echoed by Henry Oldenburg who remarked in 1668 that many English mathematicians now preferred the method of Descartes and Pell which 'seemes to them more facile and compendious [concise] than that of Oughtred'.<sup>352</sup> It must have seemed, even to most of its admirers, that the *Clavis* had served its purpose and could without dishonour be set aside, but Wallis was not ready to let it lapse into oblivion yet.

### Wallis and the *Clavis*

Wallis's admiration of the *Clavis* has already been touched on, but at this point it deserves further study, for the *Clavis* played a crucial role in Wallis's mathematical development and he in turn became the book's most ardent and lasting supporter. By his own account, Wallis read the 1631 edition of the *Clavis*

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<sup>351</sup> Robert Hooke's copy of the *Clavis* is now in the British Library, BL.529.b.19 (4,5).

<sup>352</sup> Oldenburg to Glanvill 3 October 1668, no. 970 in Hall and Hall 1965-86, IV, 75.

‘with great delight’ in 1647 or early 1648.<sup>353</sup> There are three copies annotated by Wallis in the Savile collection: *Savile Z.16*, bound in leather has just a few neatly written notes; *Savile Z.19* has Wallis’s writing on almost every page and also contains a small insert in Oughtred’s hand; *Savile Z.24* contains many of the annotations from *Savile Z.19*, transcribed and supplemented. These are beginner’s notes: rules for the four operations on negative numbers, lists of relationships between A, E, Z and X, additional diagrams for the geometrical problems, and so on.

Having worked his way through the text, Wallis’s first use of it was to take the identities involving cubes from Chapter 18:

$$Z_c = \mathfrak{Z} + 3Z\mathfrak{E}$$

$$X_c = \mathfrak{X} - 3X\mathfrak{E}$$

Wallis used these to find a solution for cubic equations, by a method similar to Oughtred’s for quadratics. His solution was identical to that of Cardano a century earlier, but then unknown to him, and he sent it off to John Smith, who had been a slightly younger contemporary of his at Emmanuel College, Cambridge, and was now lecturing in mathematics at Queens’ College. Wallis referred to his 1648 (October and November) correspondence with Smith on several occasions,<sup>354</sup> but unfortunately no copies of the letters survive. Wallis published his solution to cubic equations in 1657 in the preface to his *Adversus Meibomi*,<sup>355</sup> and wrote it out again in the course of correspondence with Collins on various algebraic topics in 1673, ending his account by saying:<sup>356</sup>

I was not displeased at this my good success upon the first attempts of a young algebraist; and the rather because I did not know but that I was the first that had made this discovery, though since I find that Cardan had been before me.

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<sup>353</sup> *Treatise of algebra*, 175.

<sup>354</sup> *Treatise of algebra*, 121,177; Wallis to Collins 29 March 1673; Wallis to Collins 8 April 1673, Rigaud 1841, II, 558, 559, 561.

<sup>355</sup> Wallis 1657c.

<sup>356</sup> Wallis to Collins 12 April 1673, Rigaud 1841, II, 564-566.

. . I was well content with my success so far, and proceeded, for my further exercise, where Mr Oughtred ends his *Clavis*, to the business of angular sections.

The ‘business of angular sections’, taken up by Wallis from the final section of the *Clavis*, became the subject of a small discourse, also sent to Smith, and this later formed Chapters 1-5 of Wallis’s *Treatise of angular sections*.<sup>357</sup> This early work was laboured and repetitive, and the results were not new (though Wallis did not know it at the time) but the value to Wallis was in what he learned from the attempt.<sup>358</sup>

And this speculation was then the more pleasing to me, because from hence I discovered the necessity, of what I did before suspect: that, in superior Equations there might be more than Two Roots; though I had not found, in Mr Oughtred, any mention at all of Negative Roots; nor, of more than Two affirmatives in any Equation.

’Tis true that *Harriot*, and (after him) *Des Cartes*, do expressly declare it; and I find that *Vieta*, was also aware of it . . But I had then seen none of these; knowing then no more of Algebra than what is in *Oughtred’s Clavis*, (from whence I had newly learned it,) and what my own thoughts did suggest from thence.

Wallis’s early study of the *Clavis* not only gave him his first crucial mathematical insights; it was also to help launch his subsequent career. When the Savilian professorship fell vacant in 1648 Wallis was unknown as a mathematician except for his correspondence with Smith, and his results on cubic equations and angular sections were his only credentials.<sup>359</sup>

Wallis probably met, or corresponded, with Oughtred shortly after he first read the *Clavis*, and by 1651 he was involved in revising and correcting the text

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<sup>357</sup> Chapters 6-9 were written about 1665, and the whole was published as an appendix to *Treatise of algebra*. For a full discussion of the treatise see Scriba 1966, 56-66.

<sup>358</sup> *Treatise of algebra*, 121.

<sup>359</sup> Wallis also claimed that he had already seen how to factorise biquadratics into quadratics, a claim discussed further in thesis Chapter 6.

for the third edition. Aubrey, who had no liking for Wallis, suggested that he even made his own self-serving contribution to the preface:<sup>360</sup>

When Mr Oughtred's *Clavis* was printed at Oxford (edition tertia with additions) the author W.O. in his Preface gives worthy characters of several young mathematicians that he enformed and amongst others of Dr Wallis who would be so kind to Mr O. to take the pains to correct the Presse, which the old gentleman doth with approval also acknowledge, and after he hath enumerated his titles: "*Viri ingenui, pii, industrii, in omni reconditiore literatura versatissimi, in rebus mathematicis ad modum perspicasis, et in enodatione explicationeque Scriptorum intricatissimis 'Zipherarum' involucris occultatorum (quod ingenii subtilissimi argumentum est) ad miraculum faelicis.*" This last of the cyphers was added by Dr Wallis himself which when the book being printed the old gentleman saw he was much he vexed at it and said that he thought he had given him sufficient praise with which he might have rested contented.

The Latin eulogy reads: 'Of a man talented, pious and industrious, most able in all abstruse literature, sharp sighted in the methods of mathematics, and fortunate in his insight into the analysis and explanation of secret writings in the most entangled and hidden of codes, (a sign of his great subtlety and skill)'. Aubrey's story was probably an exaggeration, for the style of the Latin is Oughtred's (though it is a little surprising that a staunch Royalist would be so enthusiastic about Wallis's code-breaking skills), but it is a refreshing antidote to the more generally held view that Wallis and Oughtred expressed only mutual admiration.

Wallis certainly sought Oughtred's help in 1655 in the work which he eventually published in his *Arithmetica infinitorum* later that year, but by now Wallis was breaking new ground and Oughtred failed to understand the full subtlety of what he was asking.<sup>361</sup> Nevertheless, when the book was published it was dedicated to Oughtred, who replied to Wallis with both gratitude and

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<sup>360</sup> Aubrey 1898, II, 282.

<sup>361</sup> Wallis to Oughtred 5 February 1655; Oughtred to Wallis February 1655; Wallis to Oughtred 28 February 1655, Rigaud 1841, I, 80, 85.

admiration.<sup>362</sup> Wallis continued to use Oughtred's notation throughout his life both in his private notes and his published work.<sup>363</sup>

Wallis's role in promoting the 1667 edition of the *Clavis*, despite the reservations of Collins and the booksellers, has already been noted. His greatest tribute to Oughtred and the *Clavis*, however, was to come in 1685 when he published *A treatise of algebra* and devoted fifteen of its 100 chapters entirely to the *Clavis*.<sup>364</sup> Wallis, always interested in notation, began by noting the advantages of Oughtred's:<sup>365</sup>

Thus what Vieta would have written  $\frac{A\text{Quadrate, in } B\text{cube}}{CDE\text{solid}}$ , equal to  $FG$

plane would with him be thus expressed  $\frac{A_q B_c}{CDE} = FG$

. . He doth also (to very great advantage) make use of several Ligatures, or Compendious Notes, to signify the Sums, Differences and Rectangles [products] of several Quantities.

And by this means . . he hath in his *Clavis*, a great deal of very good Geometry brought into a very narrow room; and you shall hardly find in any who have written before him so much of it delivered with so much clearness in so few words.

Wallis recognised that 'there are those who find fault with his *Clavis*, as too obscure' but argued, in words he might usefully have applied to himself, that the content

once apprehended is much more easily retained, than it were expressed with the prolixity of some other writers; where a Reader must first be at the pains to

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<sup>362</sup> Wallis 1655b, 'Dedicatio'; Oughtred to Wallis 17 August 1655, Rigaud 1841, I, 87.

<sup>363</sup> Wallis made four sheets of notes on Viète 1646 using Oughtred's notation. The sheets can still be found in the Savile Library copy, Savile N.6, and one of them is reproduced in Stedall 2000a, 54.

<sup>364</sup> *Treatise of algebra*, 67-125.

<sup>365</sup> *Treatise of algebra*, 67.



weed out a great deal of superfluous Language, that he may have a short prospect of what is material.

Having claimed here, and on other occasions, that the beauty of the *Clavis* was its brevity, Wallis now proceeded to double the length of it by quoting extensive sections and adding long explanations of his own. For example, he discussed in detail the rules for the multiplication of negative numbers which he had long ago written into his own copy of the *Clavis*. In further chapters he expanded Oughtred's treatment of fractions and proportion, and arithmetic and geometric progressions, and also set out Oughtred's method (learned from Viète) of finding numerical solutions to affected equations (for example  $R^3 - 2R^2 = 186494880$ ), as well as treating roots of binomials. Next Wallis dealt with Oughtred's 'ligatures' (his identities in Z, X, Z, X etc) and his method of solving quadratic equations (excusing the fact that the *Clavis* dealt only with quadratic equations, and only with positive roots, on the grounds that the work was meant as an introduction for, he was sure, 'Mr Oughtred could not be ignorant' that an equation of higher degree would have more roots.) Finally, he gave examples, quoted verbatim, of Oughtred's application of algebra to geometry, ending, as Oughtred did, with the work on angular sections. In this way Wallis essentially republished the entire content of the *Clavis*, generally putting the material into a better order than it appeared in the *Clavis* itself from the second edition onwards, with substantial commentary and explanation of his own, despite his criticisms in the opening paragraphs of 'the prolixity of some other writers'.

### **The final editions: 1693 onwards**

There was to be one further Latin edition of the *Clavis*, and once again Wallis was behind it. An invitation to subscribe to a new fifth edition survives among the Savile manuscripts.<sup>366</sup> It is dated April 1692 and purports to be signed by Leonard Lichfield (the younger) but the handwriting is Wallis's:

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<sup>366</sup> MS Savile 101, f. 14, reproduced in Stedall 2000a, 57.

Whereas several Learned Persons have taken Notice, That Mr Oughtred's *Clavis Mathematicae* etc has too long lain out of Print; and do complain of the many Typographical Mistakes in the last Edition. Therefore Leonard Lichfield of Oxon. Printer (in whom the Propriety of the said Copy now remains) having by the Favour and Assistance of Dr Wallis, promised a Correction of the Errors and Mistakes in the former editions of the said *Clavis Mathematicae*, does now Propose to Re-print the former, And himself Offers to those Gentlemen who shall be Pleased to Assist and Incourage this New Edition.

1. To Print it on Good Paper and in new Characters.
2. To Finish and Deliver them in Michaelmas Term, next.
3. To Deliver them well Bound (notwithstanding the present Dearness of Paper) at 3s per Book, or in Sheets at 2s-6d per Book.
4. That he may be the better Enabled to produce a good Edition of the Said Book, he Prays That some money may be paid upon Subscription, the other upon Delivery of the Book.

Leonard Lichfield

We do not know who the 'learned persons' were apart from Wallis himself. The invitation to subscribe was sent to David Gregory, recently appointed Savilian Professor of Astronomy, for his endorsement (Gregory's signature appears in the bottom left hand corner of the draft) and Wallis wrote a covering letter in which he repeated his praise of the *Clavis* and his offer of assistance.<sup>367</sup>

Sir,

I understand from Leon Lichfield that you are willing to incourage and assist him in re-printing Mr Oughtred's *Clavis*, by getting subscriptions for taking off a number of copies, at a moderate rate, when they shall be printed. Wherein I think you do very well. For the book is certainly a very good book, and the first that brought Algebra (the subtlest piece of Mathematics) into considerable

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<sup>367</sup> Wallis to Gregory 4 April 1692, MS Savile 101, f. 15.

reputation and practice. It hath been several times printed and revised with good approbation, by those who understand and apply themselves to that kind of study; and hath done a great deal of honour to our nation: But it is now quite out of print. I do not know any book which doth in so small a bulk boast so much of sound mathematicks. And it is not for the credit of our own nation, that foreigners, who have learned from him, should vent those notions under other names, without acknowledging whence they had them. The book will now fairly be acceptable both at home and abroad. But because mathematicks is not so universal a study it is not to be expected that such books should by as speedy a sale (without some other assistance) encourage a printer as common pamphlets do, which are every body's money; (and for such reason many a good book is lost.) I have promised him my assistance in correcting the editions, to free it from divers typographical faults, which in some former editions have escaped. Which to the reader will be no small advantage. And if I can be otherwise assistant I shall be willing to it. I am

Sir,

Yours to serve you,

John Wallis

This letter reveals not only Wallis's active involvement in promoting the fifth and final Latin edition, but also something of his motives in doing so. The *Clavis* was no longer needed for its mathematical content, for it was by now not only quite out of print but also long out of date: its notation had fallen into disuse and its contents were inadequate for even the most elementary understanding of algebra. Wallis, however, was concerned less with the relevance of the text than with the honour of Oughtred and the nation. Isaac Newton was persuaded to support the efforts of Wallis and Gregory, and his words echo Wallis's so closely as to imply that Wallis himself suggested them.<sup>368</sup>

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<sup>368</sup> Whiteside 1967-81, I, 15-19; 16-17. In note 7 Whiteside suggests that Newton was supporting the 1694 English translation of the *Clavis*, but Wallis's letter to Gregory refers to a corrected Latin edition.

Mr Oughtred's *Clavis* being one of ye best as well as one of ye first Essays for reviving ye Art of Geometricall Resolution and composition I agree with ye Oxford Professors that a correct edition thereof to make it more usefull and bring it into more hands will be both for ye honour of our nation and advantage of Mathematicks. Is.N.

Wallis's efforts bore fruit, and the new edition was published by Leonard Lichfield in 1693. The typographical corrections were done not by Wallis himself but by Thomas Cook, a young fellow of New College, perhaps a protégé of Wallis's, and the last in a long line of young mathematicians who had helped to correct the text of the *Clavis*.

The fifth edition was followed in 1694 by a new English translation with explanatory notes, mostly very brief, at the end of each chapter. Neither translator nor commentator was named, but the book carried a recommendation from Edmund Halley, assistant secretary to the Royal Society and editor of its *Transactions*:<sup>369</sup>

The *Clavis Mathematicae* of Mr William Oughtred is a book of so established a reputation, that it were needless to say any thing thereof. It was formerly translated by Dr Wood into English; but from an edition which has been since much bettered and augmented; and besides, the concise brevity of the Author is such, as in many places to need an explication, to render it intelligible to the less knowing in mathematical matters. This translation is new and from the fullest edition, and may be of good use to all beginners in the Analytical Art. And especially to such, who tho they be ignorant of the Latin tongue, may yet be desirous to inform themselves in Geometry: and to all such I recommend it as a very useful Treatise.

E.Halley

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<sup>369</sup> Oughtred 1694. De Morgan 1847 said that Halley made the translation, but there is no evidence to support this. Whiteside (personal communication, 14 November 1998) suggests that it would have been done by a lesser mathematician in need of the income, perhaps Joseph Raphson or John Colson.

Those 'ignorant of the Latin tongue' yet 'desirous to inform themselves' are listed on the title page as 'gagers, surveyors, gunners, military officers, mariners etc', for whom the arithmetic in the book might indeed be useful but for whom the algebraic content almost certainly was not. These readers were not quite those students of the mysteries of Classical mathematics to whom Oughtred had first offered his Ariadne's thread. The role of the *Clavis* was finally coming to an end, but it survived just a few more years: the fifth Latin edition was reprinted in 1698 with a specific acknowledgement to Wallis (now over eighty) for his revisions, and the second English edition was reprinted for the last time in 1702. Wallis died the following year, having guided the *Clavis*, whose author was born under Elizabeth I, into use in the time of Queen Anne. Ariadne's thread had wound its way across the greater part of the seventeenth century.

## Conclusion

The reasons for the popularity of the *Clavis* changed over its long life. When first published, it satisfied an urgent need for a good elementary text book soundly written, and it brought new ideas into common circulation for the first time. Small and concise and lacking any serious competitors, it became the primary text book for a whole generation of young mathematicians, some of whom were also taught personally by Oughtred and were to remember him with respect and gratitude all their lives. Two of these men, first Ward and then Wallis, gave the book its second lease of life during the political changes of the late 1640s and early 1650s.

Wallis continued to promote the book for the next fifty years both by encouraging new editions and by commenting on it extensively in his own work. As early as the 1650s, and certainly over the subsequent decade, the *Clavis* came to be seen by many as outdated, but the opinions of men like Foster, Gibson or Anderson would have carried little weight against those of the Savilian Professor of Geometry. Moreover, until the end of the 1660s good alternative texts by English writers were still not available, Gibson's *Syntaxis* remaining unaccountably unknown. It is harder to explain the final republishing of the book in both Latin and English in the 1690s, by which time it had long outlived its

usefulness, but by this time the *Clavis* and Wallis himself were both so established in English mathematics that perhaps no one could seriously question the standing of either.

The long use of the *Clavis* as a standard text book can in some ways be seen as detrimental, for it kept ideas and notation that belonged to the closing years of the sixteenth century in circulation until the turn of the next century and beyond. Oughtred's limited and limiting notation had to be abandoned before progress could be made, as many mathematicians in the second half of the century clearly saw. Nevertheless, the *Clavis* profoundly influenced the development of mathematics in England during the seventeenth century, not so much by the value of its content, but because it came into existence during a period when mathematical teaching in England was at a nadir, so that almost alone it began the revival of serious mathematical learning. Almost every seventeenth-century English mathematician or scientist of note learned their early skills from it. One can only speculate on the subsequent course of English mathematics if Moore, Ward, Wallis, Wren, Boyle, Hooke, Newton, and many lesser figures, had not had the *Clavis* to set them on their way.

Oughtred had offered the book as 'Ariadne's thread' to lead aspiring mathematicians into the mysteries of classical writings, and in this it succeeded, well beyond the circle of Oughtred's personal pupils. But the real value of the *Clavis* in the end was not as a guide to the past but as an inspiration for the future; Oughtred's key was to open doors to mathematics that Oughtred himself could have hardly imagined.

## Chapter 5

### Rob'd of glories: the posthumous misfortunes of Thomas Harriot and his algebra

#### Summary

This chapter traces the fate of Thomas Harriot's algebra after his death in 1621 and, in particular, the largely unsuccessful efforts of seventeenth-century mathematicians to promote it. The little known surviving manuscripts of Nathaniel Torporley have been used to elucidate the roles of Torporley and Walter Warner in the preparation of the *Praxis*, and a partial translation of Torporley's important critique of the *Praxis* is offered here for the first time. The known whereabouts of Harriot's mathematical papers, both originals and copies, during the seventeenth century and later are summarised. Wallis's controversial 1685 account of Harriot's algebra is examined in detail and it is argued that John Pell's influence on Wallis was far more significant than has previously been realised. The chapter ends with a reassessment of Harriot's underrated and important contribution to the development of modern algebra.

The algebra<sup>370</sup> of Thomas Harriot (c.1560-1621) has been a subject of discussion and controversy for over three centuries, thanks largely to Wallis's account in *A treatise of algebra*. Wallis devoted a quarter of the book to extolling Harriot's algebra, and repeatedly accused Descartes of having made use of it without acknowledgement. His claims for Harriot seemed so extreme and his criticism of Descartes so ill-founded that his account was never taken very seriously. The mathematician Samuel Morland wrote to Wallis after *A treatise of algebra* was published asking for clarification of Wallis's charges of plagiarism, and in his response Wallis somewhat toned down his accusations, but

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<sup>370</sup> In this chapter the term 'algebra' refers to Harriot's work on the structure and solution of polynomial equations.

held firmly to his claims for Harriot's priority.<sup>371</sup> Continental readers were less inclined than Morland to give Wallis the benefit of the doubt.<sup>372</sup> Montucla, in 1799, described Wallis's account as laughable, and a century after that Cantor dismissed it as nationalistic polemic.<sup>373</sup> In the twentieth century, Wallis has not fared any better: his own biographer described him as pompous and guilty of gross partiality, while a leading Harriot scholar wrote him off as a small-minded joker with a bad memory and only vague recall of Harriot's posthumously published *Praxis*.<sup>374</sup> Yet Wallis was a highly competent mathematician, with a deep and serious interest in the history of his subject, who knew the *Praxis* in detail. It is clearly time for a reappraisal. The real tragedy is that Wallis's account not only sullied his own reputation but, contrary to all he hoped, obscured Harriot's, so that Harriot's unique contribution to the development of algebra has never yet been properly assessed.

Nothing is known of Thomas Harriot's early background.<sup>375</sup> The appearance of his name in the Oxford University Register in 1577 implies that he was born about 1560, and the entry indicates that he already lived in Oxford, and that he took up residence in St Mary's Hall, affiliated to Oriel College. Some time after Harriot's graduation around 1580 he entered the service of Walter Raleigh, and was employed by him as navigator and scientist on a voyage to north America of 1585-86. Harriot's report of this expedition three years later, *A briefe and true report of the new found land of Virginia*, was the only thing he published during his lifetime.<sup>376</sup>

Harriot's reputation as a mathematician was already established in the early 1590s: Gabriel Harvey in 1593 named 'Digges, Hariot, or Dee' as examples of

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<sup>371</sup> Morland to Wallis 8 January 1689; Wallis to Morland 12 March 1689, Wallis 1693, 206-213.

<sup>372</sup> Prestet 1689, Preface; Baillet 1691, book VIII, 541. For further details see thesis Chapter 9.

<sup>373</sup> Montucla 1799-1802, II, 105-120; Cantor 1894-1908, III, 4.

<sup>374</sup> Scott 1938, 133-145; Tanner 1967b, 238, 270-273.

<sup>375</sup> The full length biographies of Harriot are Shirley 1983 and Stevens 1900.

<sup>376</sup> Harriot 1588.



‘profoude Mathematicians’,<sup>377</sup> and in 1594 Robert Hues announced in his *Tractatus de globis* that a further treatise could be expected from the ‘mathematician and philosopher, Thomas Harriot’. A few years later, Nathaniel Torporley, in the introduction to his *Dicliides coelometricae*, wrote:<sup>378</sup>

Our own champion has not been wanting to England. I mean Thomas Harriot, a most distinguished man, and one excelling in all branches of learning.

From 1597 Harriot had a lifelong patron in Sir Henry Percy, the ninth earl of Northumberland. The Earl was imprisoned in the Tower from 1605 to 1621 following the Gunpowder Plot (his cousin Thomas Percy was one of the ringleaders) but maintained Harriot at his home, Syon House, Isleworth, Middlesex, where Harriot was in regular contact with Walter Warner (c.1557-1643), keeper of the Earl’s library and scientific instruments, and with Robert Hues (1553-1632), tutor from 1615 to Henry Percy’s sons.

Another acquaintance of the Earl’s household was Nathaniel Torporley (1564-1632), whose praise of Harriot was quoted above. Torporley had entered Oxford in 1581 shortly after Harriot had left, and their friendship went back at least as far as 1586 when Harriot was still working for Raleigh. A letter from Torporley in Paris to Harriot in London in September 1586, shortly after Harriot’s return from Virginia, indicated that Torporley was about to meet Viète, the so called French Apollonius, for the first time:<sup>379</sup>

I am gathering up my ruined wittes, the better to encounter that French Apollon: if it fortune that either his courtesie or my boldnes effecte our conference; tomorrow beinge the daye, when I am appoynted by his Printer, as litle Zacheus to climbe the tree, to gayne a view of that renounned analist. What after followes in [his] presence I hope shortly to relate . .

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<sup>377</sup> Harvey 1593, 190.

<sup>378</sup> Torporley 1602.

<sup>379</sup> Pepper 1967a, 290.

The meeting must have gone well, as Torporley later became Viète's *amanuensis*, as mentioned by John Pell in the course of discussing the whereabouts of Viète's papers:<sup>380</sup>

I have been told here that the Englishman that, at the time of Vieta's death, served him as a scribe, under the counterfeit name of John Poltrier, being kindly offered by Vieta's heirs to take what he pleased to keep as a remembrance of him, took not a leaf of any of his writings.

The name Poltrier was a corruption of Poulterey, a near anagram of Torporley and the *alias* under which Torporley later wrote an attack on Viète. Much the same story was later repeated to John Aubrey by Robert Hooke 'on good and credible authority'.<sup>381</sup> Torporley may not have taken any of Viète's writings after his death (in 1603), but he certainly had access to them while Viète was alive. Two sheets headed 'A proposition of Vietas delivered by Mr Thorperly but no demonstration' survive amongst Harriot's manuscripts (Harriot supplied the demonstrations).

There are many other sheets which show that Harriot worked extensively on Viète's material,<sup>382</sup> and he continued to do so long after Viète died, as shown by a letter from his close friend, William Lower, in 1611.<sup>383</sup>

I fell since into Vieta's last probleme of his seconde apendicle, *Apol. Gal.*, and compared his way with yours that you last gave me: but to confesse a truth I can have my will of nether;

A letter from Lower six weeks earlier had sadly reported the death of his second son, and the loss of eighty cattle from disease.<sup>384</sup> His April letter ended:

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<sup>380</sup> John Pell to an unknown recipient 12 October 1642, Halliwell 1841, xv.

<sup>381</sup> 'Nathaniel Torporley' in Aubrey 1898, II, 263.

<sup>382</sup> 'A Proposition of Vietas delivered by Mr Thorperly', British Library Add MS 6782, ff. 482-483. Here, as with many of Harriot's sheets, the modern pagination has reversed the correct page order. Harriot's notes on Viète 1593a can be found in British Library Add MS 6782, ff. 438-481 and on Viète 1600a in Add MS 6785, ff. 50-72.

<sup>383</sup> Lower to Harriot 13 April 1611, Halliwell 1841, 41.

<sup>384</sup> Lower to Harriot 4 March 1611, Halliwell 1841, 38-40.

Since Christmas verie neere I have lost 100 beastes - Vieta's sacrifices to the witch Melusina for the invention of one probleme.

This last was a reference to Viète's remark in *Ad problema Adriani Romani responsum*: 'Moved by the beauty of this discovery, Oh divine Melusine, I have sacrificed to you a hundred sheep in place of one Pythagorean Ox'.<sup>385</sup>

A year earlier, Lower had tried hard to persuade Harriot to publish some of his discoveries.<sup>386</sup>

Do you not startle, to see every day some of your inventions taken from you; for I remember long since you told me as much, that the motions of the planets were not perfect circles. So you taught me the curious way to observe weight in Water, and within a while after Ghetaldi comes out with it in print. a little before Vieta prevented you of the Gharland for the greate Invention of Algebra. al these were your deues and manie others that I could mention; and yet to great reservednesse had robd you of these glories. . . Onlie let this remember you, that it is possible by to much procrastination to be prevented in the honor of some of your rarest inventions and speculations. . . Onlie I, because I wish you all good, wish this, and sometimes the more longlinglie, because in one of your letters you gave me some kind of hope therof.

The last of Viète's works published in his lifetime were his *Apollonius Gallus* and *De numerosa potestatum* in 1600, so Lower's testimony implies that Harriot must have been working seriously on his own algebra well before the end of the sixteenth century. There is evidence among Harriot's manuscripts that he did indeed intend to publish: there are several sections neatly written on carefully numbered sheets, which could easily have gone straight to a printer. The chief of these are Harriot's *De numeris triangularibus* (on figurate numbers)<sup>387</sup> and a treatise on equations written out in six sections lettered (a) - (f).<sup>388</sup> The hope of

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<sup>385</sup> Viète 1595a, Chapter IX, theorem III.

<sup>386</sup> Lower to Harriot 6 February 1610, Stevens 1900, 121.

<sup>387</sup> Harriot, *De numeris triangularibus*, British Library Add MS 6782, ff. 107-146.

<sup>388</sup> Sections (a) - (f) of Harriot's treatise are now scattered and disordered among the sheets of British Library Add MS 6782-6784.

publication, however, never materialised. Harriot died in 1621 and left behind him several hundred manuscript sheets on mathematics, astronomy and optics, the well-written mixed with rough working and waste, and the papers have defied almost all subsequent attempts at ordering or publication.<sup>389</sup>

Harriot's mathematical papers were left in the hands of his friends from the Earl's household: Torporley, Warner and Hues, together with John Protheroe, a Welsh landowner with an interest in astronomy, and Thomas Aylesbury, a patron of mathematical learning. (Lower had died in 1615.) These men did what they could to order and publish some of the contents but Harriot had asked too much of them and, for reasons to be explored below, none of them was able to do him justice. Three compilations of Harriot's work survive, however, all made within ten years of his death:

- (i) Nathaniel Torporley's *Congestor*, with the subtitle *Ipsam analyticam sine dubio aemulans. Felici compendio superans*, ('A compilation. Rivalling, without a doubt, the analytic art itself. By a felicitous brevity surpassing it'). This was never published but survives in two known manuscript copies.<sup>390</sup>
- (ii) A complete copy made by Torporley of Harriot's treatise on equations, in manuscript only. This is now held together with (i) in Lambeth Palace Library.
- (iii) The *Artis analyticae praxis* ('The practice of the analytic art'), published in 1631 and subsequently known simply as the *Praxis*.

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<sup>389</sup> The existing papers and their whereabouts were summarised in Pepper 1967b, 17-40. Copies of the British Library papers are now also held in the University Libraries of Durham, Oxford and Cambridge.

<sup>390</sup> Torporley, *Congestor*. Torporley's own copy was held for many years at Sion College, the home for retired clergy where he spent his final years. The Sion College MS catalogue in which it was originally listed called it *Congestor analiticus*; Anthony Wood, in his account of Torporley, referred to it as *Congestor opus mathematicum*. It was transferred, along with all other Sion College manuscripts, to Lambeth Palace Library in 1996. A second copy which I have not seen, with the title *Congestor*, is held in the Macclesfield collection.

The next part of this discussion will focus on the contents of these three works in relation to Harriot's original papers, and explore the fate of those papers in the years after Harriot's death.

### **Harriot's Will and the writing of the *Praxis***

Only a few days before his death from cancer, Harriot attempted to put his remaining affairs in order by making a Will. Later, the original was lost and the contents were known only from hearsay, leading to more than one false trail in the subsequent search for Harriot's papers. The Probate Copy was eventually found only in the late nineteenth century by the American researcher Henry Stevens.<sup>391</sup>

The exact wording in relation to the mathematical papers was as follows:

. . I Thomas Harriots of Syon in the County of Midd Gentleman being troubled in my body with infirmities. But of perfect mind and memory Laude and prayse be given to Almightye God for the same do make and ordayne this my last will and testament. . .

I ordaine and Constitute the aforesaid NATHANIEL THORPERLEY first to be Overseer of my Mathematical Writings to be received of my Executors to peruse and order and to separate the chief of them from my waste papers, to the end that after he doth understand them he may make use in penning such doctrine that belongs unto them for public uses as it shall be thought Convenient by my Executors and him selfe And if it happen that some manner of Notations or writings of the said papers shall not be understood by him then my desire is that it will please him to Conferre with Mr Warner or Mr Hughes Attendants on the aforesaid Earle Concerning the aforesaid doubt And if he be not resolved by either of them That then he Confer with the aforesaid JOHN PROTHEROE Esquior of the aforesaid THOMAS ALESBURY Esquior. (I hoping that some or other of the aforesaid four last nominated can resolve him) And when he hath had the use of the said papers so long as my Executors and he have agreed for the use afore said That then he deliver them again unto my

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<sup>391</sup> Stevens 1900, 165-178. For further details of the search for the Will see Tanner, 1967a, 1-16. The transcript used here is from Tanner 1967b, 244-247.

Executors to be put into a Convenient Trunk with a lock and key and to be placed in my Lord of Northumberlands Library and the key thereof to be delivered into his Lordships hands

It is clear that, of all his mathematical friends, Harriot thought Torporley the best fitted to understand, transcribe and edit his work. The reference to Warner and Hues, however, seems to suggest that Torporley might not have been fully acquainted with Harriot's most recent work and that Warner and Hues, his companions of later years, would be in a position to assist, with Protheroe and Aylesbury (also Executors of the Will) as the final arbiters.

Preparations for carrying out Harriot's wishes were put in train. In 1622 Torporley resigned the position he then held as Rector of Salwarpe, Worcestershire, and may have moved to one of Henry Percy's residences, Syon House in Middlesex, or Petworth House in Sussex. John Protheroe paid him a pension, and instructed his wife to continue the payments after his death (in 1624).<sup>392</sup> Later, Torporley was probably supported by Henry Percy: the Earl's household papers show a payment to Torporley in 1626, and he was certainly at Petworth in 1627. The mathematical papers handed over to Torporley were carefully listed by Aylesbury, and the list was endorsed by both Protheroe and Torporley.<sup>393</sup> It was headed:

Copyed from Mr Protheroe  
A note of the papers  
and bookes in Mr Harriot's  
trunke delivered to Mr  
Torporley

There were sixty items (plus nineteen more added later) and the first nine alone give some idea of the overwhelming task faced by the Executors:

1. Analytiques in 16 bundells
2. De Centro gravitatis 3 b.                      b. bundells  
De Jovialibus planetis

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<sup>392</sup> Shirley 1983, 413-414.

<sup>393</sup> British Library Add MS 6789, ff. 448-450; reprinted in Tanner 1969, 346-349.

3. The spots in the sun

The faces of the moon        all in one great b.

4. Of the observations of the moon, 1 great b more

5. Eratosthenes Batavus de quadrilatero in circulo, de parabola

6. Silo princeps fecit, diluvium Noachi, generatio maris et feminae with some other papers of genealogies

7. 3 b. On Vietaes zetetiques, with a few miscellaneous papers de Inclinationibus & porismatis (All these bound up in a pack thred together)

8. Of the errors in observations by Instruments which cannot be made exactly ad minutum, 1 b.

9. Certaine observations in a great b. most cleane paper

Seven books were listed under Item 60, most of them works of Viète published in his own lifetime or posthumously by Alexander Anderson:

1. Vietae { Munimen adversus Cyclom.; Adri. Romani respons.; Pseudomesolabium.; De quadrilat.; } in a little fol.

2. Lansberg, Cyclometria 4<sup>to</sup>

3. Anderson Angularium section 4<sup>to</sup>

4. Vietae Zetet. Fol. With 9 pages of loose papers.

5. Numerosa potestate, Vietae, fol. / 4 loose papers in it.

6. Archimed. A Commandino / 11 loose papers in it.

7. Vietae Apoll. gallus cum appendicula, with two bundells of papers in it de Inclinationibus and at the end, another bundell of papers pinned.

Torporley, faced with the same daunting quantity of wide-ranging and disordered material as every potential editor since, appears to have planned a five-part treatise:<sup>394</sup>

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<sup>394</sup> Torporley, *Congestor*, Sion College MS Arc L.40.2/L.40, f. 5.

	<i>Numeris Compositis vel alias primis dignoscendia</i>	}	<i>proponimus</i>
	<i>Facultate Congestita invento Harioaeo nostroque</i>		<i>nobis ad</i>
De	<i>Divisione ingeminata</i>		<i>agendur</i>
	<i>Radicalium dispensatione Hariotaea</i>		
	<i>Speciosa Logistica ab eodem perfecta a FV</i>	}	
	composition of numbers, or identification of primes	}	we propose
	a compilation of ideas discovered by our Harriot		by us to be
Of	this division repeated		treated
	Harriot's treatment of surds		
	the specious arithmetic perfected by him from Viète	}	

Torporley's *Congestor* contains the first two sections of this ambitious scheme, and a fair copy was dedicated to the Earl of Northumberland at Petworth in October 1627. The text began with a long preamble setting out Torporley's intention to produce Harriot's work in the five-part programme given above. This was followed by 'identification of prime numbers and factorisation of composites, with problems arising', including a nine page table of prime factors for numbers up to 20399. Finally, folios 26-34, under the heading *Thomas Hariotus, examinatur Stifelius de numeris diagonalibus* were an exact copy of Harriot's work on Pythagorean triples.<sup>395</sup>

The remainder of the programme was never fully completed, and certainly not by Torporley. Just when and why the work was taken over by the others named by Harriot may never be known. It has been suggested that Torporley was shocked by what he discovered of Harriot's religious views,<sup>396</sup> but this hardly holds up in the light of the work he put into the *Congestor*. It seems more likely that the Executors were concerned about the time Torporley was taking, or his ability to do the work as age and poor health began to take their toll. When Harriot died, both Torporley and Warner were close to sixty, and Protheroe's

<sup>395</sup> For further details of the history and content of the *Congestor* see Tanner 1977, 393-428.

<sup>396</sup> Jean Jacquot 1952c, 168,180.



early death in 1624 must have brought home to them the urgency of the task in hand. A later remark by Torporley that one of the editors of the *Praxis* had been 'lifted to heaven'<sup>397</sup> suggests that Protheroe was involved even before 1624, and perhaps it was decided very early on to divide the work between Torporley and the others.

A mistaken identification of handwriting has clouded the issue of Torporley's later involvement. A draft of the closing paragraph of the *Praxis*, which advised the reader of further work to follow, exists in Warner's hand<sup>398</sup> but an unknown writer has made some changes and added the endorsement: 'This will do well in this form. And I leave it to Mr Warner's discretion, whether he thinks it fit to give this monition or no, because he seemed to doubt of it'. Rigaud in the nineteenth century claimed that the second writer was Torporley and his identification has not since been challenged.<sup>399</sup> I have re-examined the handwriting, however, and it lacks the distinctive features which characterised Torporley's writing even into old age.<sup>400</sup> Torporley must be ruled out as the author of the *imprimatur*, leaving Aylesbury as the most likely alternative, but I have not so far been able to make a positive identification.<sup>401</sup>

It is possible, then, that within two or three years of Harriot's death the five-part programme set out by Torporley was shared out, with only the first half falling to Torporley, but the remainder (on surds and specious arithmetic, or

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<sup>397</sup> '... hominis per eos in coelum sublatis...', Halliwell 1841, 110.

<sup>398</sup> 'Ad mathematices studiosos', *Praxis*, 180 and British Library Add MS 4395, f. 92. In manuscript the paragraph is entitled (verso) 'Praefatio ad Opus Harrioti' and so was originally intended as a preface rather than an endnote.

<sup>399</sup> Bodleian Library MS Rigaud 35, f. 183. Rigaud's identification was accepted without question in Tanner 1967a, 9, and Tanner 1969, 342-345.

<sup>400</sup> Torporley formed the letter 'c' like an 'r' so that his algebra appears, oddly, to use the letters *a, b, r, d*. His 'r' on the other hand, resembled the Greek 'κ'. His 'e' was also written in the Greek style as 'ε'. None of these features is present in the endorsement of Warner's paragraph, and the writing is altogether more looped and flowing than Torporley's.

<sup>401</sup> There is a letter from Aylesbury to Henry Percy in British Library Add MS 4396, f. 90, but it is a copy made by Warner and so of no help in identifying Aylesbury's handwriting.

algebra) assigned to Warner. Torporley's *proponimus* ('we propose') may have been from the start a literal rather than rhetorical plural. The subtitle of Torporley's *Congestor*, ('Rivalling, without a doubt, the analytic art itself.') lends further weight to the suggestion of a division of labour. The hint of rivalry together with the bitterness with which Torporley later criticised the *Praxis*, and a comment that 'my enemies accuse me to the Master of Petworth as being ignorant of dialectic',<sup>402</sup> all give the impression that the apportioning of the work, whenever it occurred, was not without acrimony.

For whatever reason, the final part of the programme, the *Logistica speciosa*, calculation with letters, or algebra, was taken out of Torporley's hands, and eventually became the content of the *Praxis*. Torporley did, however, keep a careful copy of Harriot's treatise on equations, his six sections, (a) - (f), on the composition and reduction of polynomials. Fortunately, his manuscript was preserved in Sion College library, and is of unique importance as the only known complete copy of Harriot's treatise. Table 5 shows the correlation between Harriot's manuscripts, Torporley's copy, and the material in the *Praxis*.

Torporley made use of this copy when he later came to write his critique of the *Praxis*, his *Corrector*. On f. 42<sup>v</sup> in particular, there are notes such as '*Prob 16 et 17 mutatis signus W et 18*' ('Problems 16 and 17, with changed signs in Warner, and 18) and '*omissa W*' (missed by Warner). There are four such annotations on this page, all of them clearly referring to the *Praxis*. This is the firmest evidence yet (though previously unnoticed) that Warner was indeed the editor of the *Praxis*. It was always assumed that this was so, and friends of Warner, like Sir Charles Cavendish, John Pell or Robert Payne would have known it at first hand, but his name never appeared in print. Both Aylesbury and Warner would have known well enough that Torporley's exclusion strictly contravened the terms of Harriot's Will, which may have explained why no names were

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<sup>402</sup> ' . . . me licet hostis inter alia convitia et hoc criminetur domino Petworthiae quod essem dialecticus ignarus', Halliwell 1841, 114.

mentioned. Warner's involvement was known and remembered; Torporley's was all but forgotten.

Warner's qualifications for undertaking the work were never as strong as Torporley's. From the early 1590s until 1617 (when he was about 60) Warner was employed by Henry Percy to look after his library and scientific instruments, and afterwards received a pension until the Earl's death in 1631, but there is no contemporary evidence that he was regarded as a mathematician in his own right before he worked on the *Praxis*. Harriot's Will referred to him only for help with understanding the notation. The most detailed modern assessment describes him as 'a not too clear-thinking minor philosopher'.<sup>403</sup> This, however, was the man with whom Harriot's reputation now rested.

### **The contents of the *Praxis***

To make a proper assessment of Wallis's later account, it is necessary to look at the contents of the *Praxis* in some detail, and to compare it with the surviving manuscripts. The main text of the *Praxis* is in two parts: the first deals with the theory of equations, in six sub-sections; the second teaches practical methods of solving them numerically. The book begins, however, with a preface and eighteen preliminary definitions.

The preface is so close in style and content to the first few pages of the *Congestor* as to suggest that Torporley may have been the author. It begins with reference to Viète, and to the work he had set himself: the restoration of the analytic art which it was thought the Greeks had known but which had since been lost.<sup>404</sup>

*Artis Analyticae, cuius causa hic agitur, post eruditum illud Graecorum  
saeculum antiquitate iamdiu et incultae iacentis, restitutionem Franciscus  
Vieta, Gallus, . .*

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<sup>403</sup> Prins 1992, xviii.

<sup>404</sup> Translation by Stevens 1900, 151-152.

Francis Vieta, a Frenchman, a most distinguished man, and on account of his remarkable skill in Mathematical Science the honour of the French nation, first of all with singular genius and with industry hitherto unattempted undertook the restoration of the analytic art, of which subject we are here treating, which after the learned age of the Greeks for a long time had become antiquated and remained uncultivated; . . . But while he seriously laboured at the restoration of the old Analysis, which he had proposed to himself, he seems not so much to have transmitted to us a restoration of that science, as a new and original method, worked out and illustrated by his own discoveries. This having been enunciated in general terms, must be explained a little more at length; so that having shown what was first effected by Vieta in promoting his design, it may be more clear, what was afterwards performed by our very learned author Thomas Harriot, who followed him in these analytical investigation.

The preface then went on (as did the *Congestor*) to follow the subsequent course of Greek learning, through Diophantus and the Arabs, to Cardano, Tartaglia, Stevin and eventually Viète. The description of Viète's contribution shows an intimate familiarity with his work and ideas, and the final paragraphs, outlining the improvements made by Harriot, was written by someone who knew the mathematics of both men; a closer comparison with the Latin styles of Torporley and Warner is needed to determine the author.

Viète's first book on algebra, the *Isagoge*,<sup>405</sup> begins with a chapter in which Viète defined his terms, and the opening definitions of the *Praxis* covered much of the same material: *specious logistic*, *synthesis*, *analysis*, *zetetic*, *poristic* and *exegetic*. The *Praxis* definitions, however, were not simply copied from the *Isagoge*, but showed familiarity and confidence with the material. They also included the terms *composition* and *resolution*<sup>406</sup> and the concept of *canonical equations*, to be discussed below.

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<sup>405</sup> Viète 1591.

<sup>406</sup> These terms were used by those who followed Viète as equivalents of *synthesis* and *analysis*. See, for instance, Ghetaldi 1630.

The mathematics proper began in Section 1 which dealt, as the beginning of every sixteenth-century algebra text did, with elementary preparatory work: the four operations of arithmetic in ‘species’, or letters, (for both whole numbers and fractions), and the standard rules for simplifying equations. This was section (a) of Harriot’s treatise on equations: the examples in the *Praxis* correspond exactly (apart from some slight re-ordering) to the material headed (a) *Operationes logisticae in notis* in Harriot’s manuscripts (see Table 5). The rules for fractions were, with only minor exceptions, those in the corresponding section of the *Isagoge*, converted into Harriot’s much clearer notation. For instance, in the following example,<sup>407</sup> Harriot (in manuscript) replaced Viète’s ‘A plane’ by ‘ac’ and ‘Z square’ by ‘zz’; the latter appeared as ‘dd’ in the *Praxis*.

*Isagoge*

To add (Z square)/G to (A plane)/B

the sum will be (G in A plane) + (B in Z square) / B in G

*Praxis*

$$\frac{ac}{b} + \frac{dd}{g} = \frac{acg + bdd}{bg}$$

This example alone shows Harriot’s enormous improvements in notation and clarity. Viète had used the geometric device ‘A plane’ to maintain dimensional homogeneity (with ‘Z square’) and so ended up with a clumsy mixture of symbolism and verbal description. By replacing ‘A plane’ with the dimensionally equivalent ‘ac’. Harriot at once dispensed with both the verbal and geometric elements of Viète’s algebra and devised a notation that is still clear and relevant four centuries later.

Viète in the *Isagoge* dealt with only three of the five standard rules for simplifying equations (he omitted those for clearing fractions by multiplication and surds by squaring) and had called them *antithesis*, *hypobibasmus* and *parabolismus*. Once again, the names and the examples of the *Isagoge* were

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<sup>407</sup> Viète 1968, 338; British Library Add MS 6784, f. 324; *Praxis*, 10.

preserved in Harriot's manuscripts and in the *Praxis*.<sup>408</sup> The beauty and superiority of Harriot's notation is again outstandingly obvious:

*Antithesis*

[addition to each side]

*Isagoge*

Let it be given that A square minus D plane is equal to G square minus B in A. I say that A square plus B in A is equal to G square plus D plane and that by this transposition under opposite signs of conjunction the equation is not changed.

*Praxis*

Let	$aa - dc = gg - ba$
To be added to each	$+ ba + dc$
Whence	$aa + ba = gg + dc$

*Hypobibasmus*

[removal of excess powers of the unknown]

*Isagoge*

Let it be given that A cube plus B in A square is equal to Z plane in A. I say that by hypobibasm, A square plus B in A is equal to Z plane.

*Praxis*

Let	$aaa + baa = dca$
Then	$aa + ba = dc$

*Parabolismus*

[reduction of the leading coefficient to unity]

*Isagoge*

Let it be given that B in A square plus D plane in a is equal to Z solid. I say that by parabolism A square plus (D plane)/B in A is equal to Z solid/B. For that means to have divided all the solids by the common divisor B, by which it is certain that the equation is not changed.

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<sup>408</sup> Viète 1968, 342-345; British Library Add MS 6784, f. 325 and *Praxis*, 11.

*Praxis*

Let  $baa + dca = gcd$

Then  $aa + \frac{dca}{b} = \frac{gcd}{b}$

Section 1 of the *Praxis* thus effectively covered all the main mathematical content of the *Isagoge* but in Harriot's notation rather than Viète's. The only brief but important addition was a note on the new signs <, > for inequality.<sup>409</sup> In his manuscripts Harriot wrote the symbols <, =, > with two additional short vertical strokes in each, but Warner in copying them simplified them to the forms now in common use.<sup>410</sup>

Here the close similarity between the *Praxis* and Harriot's manuscripts comes to an end. In Harriot's plan, section (b) on surds follows next, but Warner proceeded directly to the contents of (d), *De generatione aequationem canonicarum* ('The generation of canonical equations'). This was the section where Harriot began to develop his profound and far-reaching insight that polynomial equations could be built up, or composed, of linear or quadratic factors such as  $(a - b)$  or  $(aa - df)$ . (Here, as elsewhere, Harriot used  $a$  for his unknown quantity where we would now usually write  $x$ .) Step by step, starting with the quadratic  $(a - b)(a + c) = 0$ , Harriot set out to compile a list of 'canonical equations'. From the nature of their composition it was easy to see not only what the roots of each equation must be, but also the relationship of the roots to the various coefficients. Hence, any given equation could be compared with a 'canonical', and one would immediately have important information about the number and nature of its roots.

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<sup>409</sup> *Praxis*, 10. For Harriot's introduction of inequality signs see Tanner 1967b, 241, 278-279.

<sup>410</sup> The *Praxis* helped to standardise the use of the = sign. Warner's simplified < and > can be seen in British Library Add MS 4394, f. 392.

Here is Harriot's first example of his method, from the sheet he himself numbered d.1).<sup>411</sup>

Let  $a = b$  in the multiplication 
$$\begin{array}{r} b - a \\ \underline{c + a} \end{array}$$

therefore 
$$\begin{array}{r} b - a \\ \underline{c + a} \end{array} = bc - ca$$
  

$$+ ba - aa = 00$$

therefore  $bc = -ba$

$+ca + aa$

which will give  $a = b$  but not  $c$  nor anything other than  $b$

[The dividing lines were drawn in by Harriot to separate each section of working]

If  $a = b$

we will have  $bc = -bb$

$+bc + bb$  which is indeed the case

If  $a = c$

we will have  $bc = -bc$

$+cc + cc$

$2bc = 2cc$

therefore  $b = c$  which is against what was proposed

Therefore  $a = b$  and not  $c$

If  $b = c$  the first degree term may be taken away

and we will have:  $bb = aa$

and:  $a = b$

(1)

In the original Latin this piece of work is even more concisely and beautifully written than it appears here. For those unfamiliar with Harriot's style it should be noted that he often used a vertical rather than horizontal layout, and the results of

<sup>411</sup> British Library Add MS 6783, f. 183. Harriot marked his sheets in the top right or top left corner as d.1), d.2), d.3) etc.



the first multiplication,  $(b - a)(c + a)$ , are given with the ‘constant’ term  $bc$  first, then the terms in  $a$ , listed vertically, and finally the single term in  $aa$ . The same layout can be used for much longer multiplications and shows clearly the relationship of each coefficient to the possible roots. Note also the use of 00 to preserve the homogeneity of the terms.

It has been observed many times that the *Praxis* consistently ignored negative roots,<sup>412</sup> and so it is of interest to note that here, at this early stage, so did Harriot himself. Underneath ‘let  $a = b$ ’ in the first line of (1) he could also have written ‘or let  $a = -c$ ’; there is evidence that he may initially have done so, for in the manuscript something has been heavily crossed through and is no longer legible. But the way Harriot proved that  $a$  could not equal  $+c$  indicates that he was concentrating here only on positive roots. Not until a later sheet, d.7.2<sup>0</sup>),<sup>413</sup> did Harriot specifically write a negative root,  $a = -f$ . This will be discussed in more detail below.

Harriot’s method here and throughout his section (d) was a model of clarity, but for reasons we can never know, Warner chose not to follow it. Instead, he based his exposition on Definitions 14, 15 and 16 from the Introduction. This change will prove important when we come to analyse Wallis’s account later, and so the definitions underlying Warner’s work are given here for reference. The rather lengthy wording of the *Praxis* has been abbreviated into modern English but the algebra is unchanged.<sup>414</sup>

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<sup>412</sup> Cajori 1928, 317-320.

<sup>413</sup> British Library Add MS 6783, f 204. Harriot used ‘2<sup>0</sup>’ (‘secundo’) to indicate that sheet d.7.2<sup>0</sup>) was a later version of d.7).

<sup>414</sup> *Praxis*, 4-5.

*Definition 14:*

Originals of canonicals. Equations of this kind are made by multiplication from binomial roots and by rearrangement.

$$\begin{array}{l} a + b \\ \hline a - c \end{array} = \begin{array}{l} aa + ba \\ - ca - bc \end{array}$$

*Definition 15:*

Primary canonicals are those established by derivation from the originals. For example:

$$\begin{array}{l} aa + ba \\ - ca = +bc \end{array}$$

*Definition 16:*

Secondary canonicals are established by reduction from the primary ones. By removing some of the incidental degrees they become secondary.

$$aa = + bb$$

The definitions in this form have not been found in the manuscripts, and it is not clear whether it was Torporley or Warner or Harriot himself who wrote them. Warner, however, made use of them. Using them as 'headings', he copied under each one the relevant material from the manuscripts, a routine task which required no great understanding, though it involved combing through the material three times instead of just once. As a result, Harriot's unified procedure for each equation was dismembered and scattered by Warner over no fewer than four separate sections of the *Praxis*: material corresponding to Definition 14 was in the first half of Section 2, and to Definition 15 in the second half of Section 2; Definition 16 was covered in Section 3 and the remaining material, not covered by any of the definitions, was in Section 4.

Every detail of Harriot's manuscripts points to a confident mastery of his subject; unhappily the same cannot be said of Warner. Harriot, dealing specifically with positive roots, had omitted cases such as  $(a + b)(a + c)$  which could not be turned into primary canonicals without admitting negatives. Warner,

lacking Harriot's clear purpose, introduced them, but was then forced to drop them again, so that he began Section 2 with 32 equations but ended with only 18. To compound the confusion he repeatedly re-ordered the equations as he moved from one section (or part section) to the next, further destroying any sense of unity or continuity.

One particular group of equations which gave Warner trouble must be noted here. Section 3, like Definition 16, was based on the fact that some terms of a polynomial equation will disappear if certain relationships hold between the roots (there will be no term in the second highest power, for instance, if the sum of the roots is zero). The whole of Section 3 was devoted to listing such relationships and their effects. From a cubic equation, either of the terms in  $a$  or  $aa$  might disappear, and the *Praxis* gave seven examples for different cubics. The remaining thirteen examples were all of biquadratics, inevitably longer and more difficult, but Warner continued valiantly to write out the conditions and the working in full each time until he faced defeat in the final three problems: 19, 20 and 21. These required the elimination not just of one term, but of two, and therefore needed two independent relationships between the roots. Warner gave only one relationship for each, omitted any working, and added an apologetic note: 'The reductions of these equations, since they are delivered more obscurely in manuscript, must be referred to a better enquiry.'<sup>415</sup> Harriot's working was indeed a little obscure: bored with writing out full solutions to quadratic equations, he had begun to write such things as

$$d = \frac{bbc + bcc}{2bb + 2bc + 2cc} - \sqrt{\phantom{x}}$$

$$d = \frac{bbc + bcc}{2bb + 2bc + 2cc} + \sqrt{\phantom{x}}$$

leaving the reader to fill in the empty space under the square root sign.<sup>416</sup> This was not, however, mere idleness on Harriot's part: he knew from experience that the terms ' $-\sqrt{\phantom{x}}$ ' and ' $+\sqrt{\phantom{x}}$ ' would cancel out as soon as he added the two solutions a line

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<sup>415</sup> *Praxis*, 46.

<sup>416</sup> British Library Add MS 6783, f. 174.

or two later. Poor Warner, however, was left with an ungainly square root to fill in. Even worse, if he managed to do it, he would have seen that the expression under the square root sign was negative: Harriot was dealing here with what we would now call complex conjugates. Harriot may have been at ease, but Warner was decidedly not, and he chose to avoid the pitfalls of problems 19-21. Both Torporley and Wallis, for different reasons, were to take up Problem 19 later, and we shall return to it.

Having scattered Harriot's examples over Sections 2, 3 and 4, Warner began in Section 5 to show how the number of positive roots of any equation could be found by comparing it with an appropriate 'canonical'. Except for one stray quartic, he limited himself to cubics, especially  $aaa - 3baa = 2ccc$  for the three cases  $c > b$ ,  $c < b$ ,  $c = b$ .<sup>417</sup> These three cases corresponded to three different canonicals, as Harriot showed by proving a series of inequalities, for instance  $(p+q+r)^3 > 27pqr$  (for  $p$ ,  $q$  and  $r$  positive and not all equal). There are significant differences between the proofs of these inequalities in the *Praxis* and those in the surviving manuscripts.<sup>418</sup> Harriot started from the well known fact that the arithmetic mean of two different, positive numbers is greater than their geometric mean. Warner attempted to prove this but did so by a circular argument in which he assumed it was true in the first place. In the more difficult inequalities the manuscript proofs began with what was to be proved and worked back to a simpler, known truth (a process of *analysis*), but Warner exactly reversed this process and worked from simple to more complex (a classic example of *synthesis*).<sup>419</sup>

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<sup>417</sup> This material was mainly a summary of Harriot's discussion of cubics on his sheets e.8) and e.9). The two final lemmas were from e.28) and e.29) and there was a little additional material from elsewhere in section (e).

<sup>418</sup> British Library Add MS 6783, ff. 106-107, 184-185; *Praxis*, 78-86.

<sup>419</sup> Harriot's manuscripts contain a number of examples which are explicitly worked by both *synthesis* and *analysis*; in each case the working of the *synthesis* is exactly the reverse of the working by *analysis*. It is therefore possible that Harriot worked his inequalities in both directions and that Warner took his (*synthetic*) versions from sheets that are now lost.

The sixth and final section of Part I was concerned with the elimination of the second term from a polynomial equation by a linear transformation of the root (that is to say, deliberate elimination, not the accidental elimination arising from the nature of the roots, which had been dealt with in Section 3.) The method, originally devised by Cardano, had been developed briefly by Viète in the first part of his *De aequationem recognitione*, and more extensively in the later second part, the *Emendatione tractatus duo*, where he treated all eleven cases of removing the square term from a cubic.<sup>420</sup> Problems 1-11 of Section 6 of the *Praxis* covered exactly the same ground. Problems 12-14 arrived at the solutions, first given by Cardano but now in Harriot's notation, for cubics of the form  $aaa \pm 3bba = 2ccc$ , illustrated with brief numerical examples, the only ones in Part I (from Harriot's sheets e.5) and e.6)). The remaining problems, 15-34, dealt with the removal of the cube term from a quartic. This had not been treated in Viète's published work, and may have been Harriot's own extension of the method. All the examples are to be found in Harriot's section (f), though the notation was changed by Warner.<sup>421</sup> Each of the nineteen cases was worked at full length, repetitively and without linking text.

Finally came the long Part II (about one third of the book) on solving equations numerically. These were essentially Viète's methods, transcribed by Harriot into his own notation. On the contents page of the *Praxis*, and again at the end of Part I, it was stated that such numerical solution was the 'principal skill' of the analytic art and that the aim of Part I was to prepare the way for Part II. It was never made very clear to the reader how this was so, but the statement has been accepted by later commentators.<sup>422</sup> It is contradicted, however, by Harriot's own

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<sup>420</sup> Viète 1615a. The elimination of terms from a cubic is in Chapter 1 of the second part, *Emendatione tractatus duo*. The eleven cases arise from the different combinations of positive and negative coefficients.

<sup>421</sup> Harriot's section (f) comprised at least six different sequences of manuscript sheets, perhaps written out at different times. Torporley used the Greek letters  $\alpha - \zeta$  to distinguish them.

<sup>422</sup> On this point, Tanner 1967b, 241, wrote this extraordinary passage (my italics): ' . . . the true nature of the work has been completely obscured. It is "advanced arithmetic" with a *minimum of*

ordering, in which he placed the *De numerosa potestatum* as section (c) immediately after his section (b) on surds. The more difficult theoretical content of sections (d), (e) and (f), then *followed*. Warner reversed this order (as did Torporley in his copy). A brief summary of the relationship of the *Praxis* to the manuscript material is given in the following table:

<i>Praxis</i>	<i>Harriot's manuscript sections</i>
Part I Section 1	(a)
Sections 2 - 4	(d)
Section 5	mainly (e.8,e.9,e.28,e.29)
Section 6	(e.5,e.6) and material from (f)
Part II	methods but not material of (c)

### **Torporley's refutation of the *Praxis***

When the *Praxis* appeared in print Torporley attacked it bitterly in a piece entitled *Corrector analyticus* but died in 1632 before he could complete it. His unfinished text was the third and last of his manuscripts to be preserved at Sion College; no translation has ever been published.<sup>423</sup> Torporley's rambling prose has here been abridged and put into modern English, to convey the main points of his argument.

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*algebraic foundation, nothing like algebra in its own right* . . . Harriot's executors had naturally started with the most elementary and practical part of his mathematical theory, which he taught alike to aristocratic amateurs for their sport and to young sea-captains for their use in applications.' Some sea-captains!

<sup>423</sup> Torporley, *Corrector*, Sion College MS Arc L.40.2/L.40, ff. 7-12. The title is translated in Stevens 1900, 5. The remaining text is printed in Latin, though with many errors, especially in the mathematics, in Halliwell 1841, 109-116.

*Corrector Analyticus artis posthumae Thomae Harrioti*

<i>Ut mathematici eximii, perraro</i>	}	<i>errantis</i>
<i>Ut philosophi audenti, frequentius</i>		
<i>Ut hominis evanidi, insigniter</i>		

An Analytic Correction of the posthumous work of Thomas Harriot

As an exceptional mathematician, one who very seldom	}	erred
As a bold philosopher, one who more often		
As a mere human, one who conspicuously		

For the more trustworthy refutation of the pseudo-philosophic atomic theory  
revived by him, and other strange notions deserving reprehension and  
anathema.

A compendious warning with examples by the aged and retired Nathaniel  
Torporley.

I am going to take in hand this posthumous analytic tract just published. There are three things to consider, not just disputed and ambiguous, but lying and false, and therefore more carefully to be refuted. The work is imperfect and defective, and while the author himself had not yet perfected these things, they were carefully chosen and approved by him to be conveyed faithfully to posterity. The editors are not excused by their statement that this first part is merely an introduction, for Harriot's own findings are here mixed with what he has borrowed from elsewhere; his own work has been compressed, though it truly requires a volume of considerable size. The third thing, which in fact must be considered first, is that Harriot's teaching is so unusual, that it would be laudable to make it public. But this recent writing is less like an author teaching his readers than like students repeating their lessons by rote. What is taken from elsewhere, if not rightly understood, degenerates into falsehood. Let us see what Harriot himself intended. First we speak of his method, to compare it with what was actually published (or omitted); it is impossible not to complain that

they have utterly changed what so badly needed explanation, retaining neither his order nor his words. What was most worthy of praise they have spread amongst the random findings of an illiterate. Those already lifted to heaven, and those who in the end wrote the work, so changed it that scarcely a trace of it remains, a silent confession that they needed assistance. Harriot's method was this.

First a treatise on surds, not separated from the Analytic Art but part of it. If it is useless to the numerical exegesis, why mention it? And if mentioned, it is not useless, so why omit it?

He also added, as a prelude to the analysis, the square and cube roots of binomials (27-28 sheets). He was accustomed, like a learned lawyer, to combine different sections of his documents when he needed to make an argument, and interspersed his propositions with numerical working.

If they realised this, they would have done better. The basic operations of arithmetic in Harriot's notation should be where they begin the definitions, under the title, with the examples not far from there. On the analytic art itself, Harriot was writing three parts. The first was the generation of canonical equations, from 21 sheets lettered (d) (with two appendices on the multiplication of roots).

The second part, with the title *De resolutione aequationum per reductionem*, is in section (e) (29 sheets), and (f  $\alpha$ ) (7 sheets), (f  $\beta$ ) (7 sheets), and (f  $\gamma$ ) up to (f 18  $\gamma$ ) (with two lemmas they have missed). Then (f  $\delta$ ) (8 sheets), (f  $\epsilon$ ) (4 sheets), (f  $\xi$ ) (4 sheets). Afterwards 9 sheets containing old solutions reworked by Harriot's method.

The third part is like Viète in his book, so I am not inclined to disagree with it. Not all of Viète's work is in a single example or paragraph [here Torporley discusses several sheets lettered (b) and (c), separating out Harriot's examples from Viète's].

This is it in general; and how far one has to seek for it, as easy and clear in Harriot as it is dismembered by them. It is clear that of the three parts, that on numerical solution is the most like Viète, as Harriot said himself, little changed. How little? They have added something to the brevity of the precepts, but taken nothing from the business of the operation. Viète is to be recognised as the parent of most of Part II. All that remains of Harriot is Part I.



From his first hypothesis (that any quantity multiplied by nothing gives nothing) he has deduced his universal canon. This can hardly have happened without the leading of divine providence. He [Harriot] who insisted on the axiom *ex nihilo, nihil fieri* ['from nothing, comes nothing'] has damned himself by his own error, since he has produced from nothing such beautiful miracles of art. Though mortal, from nothing he has generated the immortal. Why then may not God the all-powerful, the sum of all wisdom, create from that same nothing? But lest declamation grow stronger than demonstration, let us come to what we censure and condemn in this posthumous treatise. We will examine just one absurdity, as an example of all the others (without judging them less important). In this we imitate Aristotle: from the crime of one, learn the errors of all.

In the said work, in Section 3, are three consecutive problems 19, 20 and 21, which the notes imply are not well understood and which are referred to a better inquiry. [Example 19 is here quoted verbatim from the Praxis.]

These three reductions demonstrate the reduction of quadrinomials to binomials and seem to Harriot's editors to demonstrate his practice. But to me they fail to reveal Harriot's most profound work. The missing reduction is to be found in proposition 11 among the originals.<sup>424</sup>

Let us establish Harriot's vision more carefully. In Harriot's sheet d.7.2<sup>0</sup> ) we have this, which we put forward because his spokesmen have been shabby in it. [Here Torporley gave Harriot's quadrinomial  $(a - b)(a - c)(a - d)(a + f) = 0$ , slightly different from the equation in Problem 19 which is  $(a - b)(a - c)(a + d)(a + f) = 0$ .]

To correct these problems I offer a syllogism, though my enemies accuse me to the master of Petworth [the Earl of Northumberland] as being ignorant of dialectic.

To reduce a quadrinomial to a binomial, it is necessary that in the terms to be removed the positive parts must equal the negative parts. But in these problems this is impossible.

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<sup>424</sup> The reductions are actually in d.10) to d.12), British Library Add MS 6783, ff. 172-174 (reversed).

Therefore: in these problems it is not possible to generate a binomial from a quadrinomial.

Against the major proposition they have erred childishly; against the minor proposition Harriot has erred thoughtlessly.

To correct the first error is not hard work. It is impossible, using one equation, to take away more than one term. Which Harriot knew. For in each case he gave two equations for two terms. This deals with the lesser argument; the greater argument needs more effort and requires the following lemmas.<sup>425</sup>

[Here Torporley embarked on a geometrical argument headed Lemma 1 but never completed it.]

Putting aside the tone of complaint and Torporley's rambling language, the most striking thing about the *Corrector* is its essential accuracy. If one accepts his claim (and there seems no reason to doubt it) that Harriot's lettered sections (a) - (f) were those he intended for publication, then Torporley was right in both the thrust and the detail of his argument. The work in the *Praxis* had every appearance of having been copied without understanding and of having been disordered, dismembered and scattered. Part (b) on surds was missing and sections (d), (e) and (f) had become almost unrecognisable.

Torporley's mathematical arguments at the end deserve closer scrutiny, especially his introduction of sheet d.7.2<sup>0</sup>).<sup>426</sup> This sheet was not copied by Torporley, but in the surviving manuscripts it is to be found separated from the rest of section (d), and was perhaps extracted by Torporley specifically for the purpose of writing the *Corrector*. This sheet, and another marked d.13.2<sup>0</sup>),<sup>427</sup> are both of very great interest. The suffix 2<sup>0</sup> [2<sup>nd</sup>] indicates, and the content confirms, that they contain further developments to material in d.7) and d.13) and since Harriot's sheets are undated they offer a rare and extremely valuable insight into the development of his ideas.

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<sup>425</sup> Torporley's use of 'major' and 'minor' in the Latin at this point reverses his use of the same terms in the preceding paragraph: the 'lesser' (*minor*) argument is the *major* proposition.

<sup>426</sup> British Library Add MS 6783, f. 204.

<sup>427</sup> British Library Add MS 6783, f. 156, reproduced in Lohne 1966, 195-196.

Taking sheet d.13.2<sup>0</sup>) first, it contains, among other examples, the following:

$$\begin{array}{l}
 b - a \\
 c + a \\
 \hline
 df + aa
 \end{array} = bcdf + bdfa - dfaa + baaa \\
 - cdfa + bcaa - caaa - aaaa = 0000 \\
 \text{Ergo } bcdf = - bdfa + \dots + aaaa \quad a = b \\
 a = -c \\
 aa = -df \\
 a = \sqrt{-df} \quad (2)$$

The sheet is now found, at first unexpectedly, in one of the sequences of section (f).<sup>428</sup> Inspection reveals that sheet f.14) which follows it, makes direct reference to d.13.2<sup>0</sup>), so the placing was clearly deliberate, and direct evidence of the way Harriot, 'like a learned lawyer', combined arguments from different parts of his papers.

Sheet d.7.2<sup>0</sup>) has already been mentioned as containing the first evidence (with d.13.2<sup>0</sup>) ) of an explicit negative root. Of even greater interest, however, in the context of Torporley's argument, is Harriot's treatment in it of the equation  $(a - b)(a - c)(a - d)(a + f) = 0$ . Harriot's objective was to remove the square and cube terms, for which the necessary conditions are:

$$b + c + d = f \quad (i)$$

$$bc + bd + cd = bf + cf + df \quad (ii)$$

These were to be solved for  $d$  and  $f$  in terms of  $b$  and  $c$ , and it was soon apparent that if  $b$  and  $c$  were real (as we would now describe them), then  $d$  and  $f$  must be imaginary.<sup>429</sup> Since  $b$ ,  $c$ ,  $d$  and  $f$ , like every coefficient of every equation in all mathematics up to then, were supposed to be positive and real, this should have stopped Harriot in his tracks. It did not. He went on to substitute his imaginary

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<sup>428</sup> The sequence which was labelled by Torporley as (f.γ).

<sup>429</sup>  $d, f = \frac{b+c \pm \sqrt{-3bb-2bc-3cc}}{2}$ .

values of  $d$  and  $f$ , and arrived correctly at the reduced equation:

$$\begin{aligned} bbbc &= bbba \\ bbcc &+ bbca \\ bccc &+ ccca \\ &+ ccca - aaaa \end{aligned}$$

Harriot had taken the quite remarkable step of moving between equations with real coefficients by way of imaginaries.<sup>430</sup> For Torporley this was too much, and it must be what he meant by the ‘thoughtless error’ Harriot was supposed to have committed and, perhaps, the ‘strange notion’ of the title page. Harriot was sailing seas over which neither Torporley nor Warner could follow.

What else Torporley wanted to say we can never know, for he never completed his refutation; he had already described himself as *jam senex et jam moriturus* (‘now a worn out old man approaching death’), and towards the end of the *Corrector*, his writing weakened visibly and petered out. His despairing and unfinished attack on the *Praxis* was the last thing he ever wrote, and those for whom it was intended may never even have known of its existence.

### The Harriot and Warner papers after 1631

Under the terms of Harriot’s Will, his mathematical papers should have been returned, after Torporley had finished with them, to the Earl of Northumberland for safe-keeping. All the available evidence supports the hypothesis that the papers used by Torporley were indeed those discovered at Petworth in 1784 and now in the British Library: Torporley’s references and descriptions can all be verified from the manuscripts. A particular feature of certain sections in the British Library manuscripts is the reverse pagination which occurs frequently, easily explained if one imagines someone copying the sheets, as Torporley certainly did, and laying each aside face up. Torporley’s copy can be dated from his references to Warner; these may have been added later, but appear to have been included when the copy was first made, which would indicate that it was done after the text of the *Praxis* was written, though possibly before it was finally

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<sup>430</sup> Imaginary quantities arise in Problems 19, 20 and 21 in exactly the same way.

printed in 1631. The papers were presumably returned to Petworth just before or after Torporley's death in 1632, and the death of Henry Percy in the same year would explain well enough why the papers lay so long forgotten.

Torporley's is the only known complete copy of Harriot's treatise on equations. However, it was common practice at the time, and for many years afterwards, for new mathematical results to be disseminated through hand-written copies, of single sheets or whole treatises, to the relatively small number of those who understood such things. To assess the true extent of Harriot's influence on seventeenth-century mathematicians it is important to consider carefully how far his work was circulated in this way.

There is certainly evidence that Warner and Aylesbury kept some of Harriot's papers in the hope of further publication. A letter from Aylesbury to Percy in April 1632 confirmed their intention:<sup>431</sup>

I purpose, God willing, to set forth other peeces of Mr Harriot, wherein, by reson of my owne incumbrances I must of necessity desire the help of Mr W., rather than of any other; whereunto I find him redy enough, because it tends to your Lordship's service, and may the more freely trouble him, yf he receive some little encouragement from your Lordship . .

The Earl's death three months later put an end to hope of further funding from that source, but not, apparently to the plans of Aylesbury and Warner. In 1635, Samuel Hartlib recorded in his *Ephemerides* (his notebooks on scientific matters) Henry Gellibrand's remark that 'Mr Warner hase all Harriot's manuscripts' and was 'setting some of them forth'.<sup>432</sup> Copies of fragments or longer sections of Harriot's mathematics are scattered throughout Warner's papers. The only one of relevance to the story of Harriot's algebra is a sheet containing two paragraphs marked d.4) and e.10). Both are copied exactly from Harriot's unpublished pages

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<sup>431</sup> Aylesbury to Northumberland 5 April 1632, Halliwell 1841, 71, but wrongly supposed by Halliwell to be from Torporley to Northumberland.

<sup>432</sup> Clucas 1991, 45.

d.4) and e.10).<sup>433</sup> Warner, of course, had ample opportunity to copy all of part of Harriot's treatise both before and after publication of the *Praxis*.

In 1639 Hartlib wrote that John Pell had been working on some of Harriot's problems and that 'Sir Thomas Alesbury promised to let him have Harriot's papers but hee did solve them without them'.<sup>434</sup> Through Aylesbury, Pell came to know Warner and began to collaborate with him on tables of 'analogies' (antilogarithms). To this end constant difference tables first devised by Harriot<sup>435</sup> are found repeatedly throughout the papers of both. Pell also made several references to material from the *Praxis*, which he clearly knew well.<sup>436</sup>

Sir Charles Cavendish, acquainted with both Warner and Pell, and a longstanding collector of mathematical books and manuscripts, also took a keen interest in Harriot's work. Among his surviving papers is a complete copy in his own hand of Harriot's unpublished *De numeris triangularibus*.<sup>437</sup> Cavendish referred here and elsewhere to Harriot's 'loose papers', for example: 'this 4<sup>th</sup> manner, from Mr Hariot's loose papers, is not from his booke of triangular numbers.'<sup>438</sup> For both Cavendish and Pell, the source of the 'loose papers' was clearly either Warner or Aylesbury, and so it is of some importance to try to trace the fate of the papers of these two men.

In the case of Aylesbury, the trail soon becomes cold. Aylesbury's position as baronet and Master of the Mint had enabled him to become 'a lover and encourager of learning and learned men', but he was cashiered as a Royalist in

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<sup>433</sup> British Library Add MS 4394, f. 392. The original Harriot sheets are Add MS 6783, ff. 180, 108.

<sup>434</sup> Clucas 1991, 45.

<sup>435</sup> Harriot used the method of constant differences to generate figurate numbers. Warner and Pell used it for the interpolation of tables.

<sup>436</sup> British Library Add MS 4413, f. 224; Add MS 4415, f. 83; Add MS 4420, ff. 19-22.

<sup>437</sup> British Library Harley MS 6083, ff. 403-455.

<sup>438</sup> British Library Harley MS 6083, f. 404. Other examples are to be found in Harley MS 6002, ff. 4, 44 and Harley MS 6083, ff. 403<sup>v</sup>, 429<sup>v</sup>.

1642 and stripped of his fortune and property.<sup>439</sup> An anonymous, post-Restoration, account of the men around Percy included the passage:<sup>440</sup>

Sir Thomas Aylesbury Master of Requests and a great officer of the Mint told me that he had warriners [Warner's] book [on the circulation of the blood] and that I should have it, but coming to London he found his Library, wherein were many rare and curious books, plundered, and that amongst the rest taken away.

It seems likely that any Harriot papers still in Aylesbury's possession met the same fate. Aylesbury left England for the continent in 1649 and died abroad in 1657, and later searches through the family papers failed to reveal anything relating to Harriot.

The fate of Warner's papers was more complex, but can be pieced together from a number of contemporary references. After Warner's death in 1643, Cavendish made enquiries on the tables of antilogarithms, or 'analogics' which Warner and Pell had planned to publish. Pell replied that he feared Warner's papers were lost:<sup>441</sup>

And first for Mr Warner's analogickes, of which you desire to know whether they be printed. You remember that his papers were given to his kinsman, a merchant in London, who sent his partner to bury the old man: himselfe being hindred by a politicke gout, which made him keepe out of their sight that urged him to contribute to the parliament's assistance, . . . Since my comming over [to Amsterdam], the English merchants heere tell me that both he and his partner are broken, and now they both keepe out of sight, not as malignants, but as bankrupts. . . . In the meane time I am not a little afraid that all Mr Warner's papers, and no small share of my labour therein, are seized upon, and most unmathematically divided between the sequestrators and creditors, who will, no doubt, determine once in their lives to become figure-casters, and so vote them all to be throwen into the fire, if some good body doe not reprieve them for

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<sup>439</sup> Wood 1691, col 305.

<sup>440</sup> Bodleian Library MS Rawlinson B.158, f. 153.

<sup>441</sup> Cavendish to Pell 26 July 1644; Cavendish to Pell 8/18 August 1644; Pell to Cavendish 7 August 1644, Halliwell 1841, 78-81.

pye-bottoms, for which purposes you know analogicall numbers are incomparably apt, if they be accurately calculated.

Fortunately, some of Warner's papers escaped both fire and pie crust, and were inherited by Nathaniel Tovey, his nephew by marriage. In the early 1650s Tovey passed them on to Herbert Thorndike who had a keen interest in mathematics and did what he could to get the papers seen by those who might best understand them. In 1650 Hartlib remarked in the *Ephemerides* that Seth Ward, Oxford professor of astronomy and a friend of Thorndike, 'is to set out the mathematical and other workes of Warner conc[erning] coyne etc.'<sup>442</sup> Ward's main interest appears to have been in Warner's optics, which he later accused Hobbes of plagiarising.<sup>443</sup> At the time, Ward shared the Savilian Professors' study with Wallis, but thirty years later, in a letter to Aubrey, Wallis was vague about Ward's involvement and whether the papers were Harriot's originals or Warner's copies (or both). Knowing Aubrey's propensity for gossip, Wallis may have been deliberately guarded, but his remarks did suggest that some Harriot manuscripts might have been amongst the Warner papers in Ward's care:<sup>444</sup>

I have formerly heard that they [Harriot's papers] had been, at some other time, in Mr Hobbes' hands. That they had been at some other time, in Dr Pell's hands. And that some time they had been in the hands of the present Bishop of Salisbury [Ward]. But it is many years since I heard any thing of certainty where they are: and feared they might have perished.

Collins later reported that Thomas Gibson had borrowed some of Warner's papers from Thorndike in 1650,<sup>445</sup> and Gibson's *Syntaxis* published in 1655 drew explicitly on Harriot's work. In 1652 Thorndike sent the papers on 'analogics' to

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<sup>442</sup> Clucas 1991, 44 n. 88.

<sup>443</sup> Ward 1654, Introduction and 247.

<sup>444</sup> Wallis to Aubrey 20 July 1683, MS Aubrey 13, f. 242.

<sup>445</sup> Collins to Gregory 25 March 1671, Rigaud 1841, II, 219. See notes 323, 344.



Pell, but a month later Pell told Thorndike that 'I must change my resolution of putting out Mr Warner's writings because they [are] so incomplete.'<sup>446</sup>

Some of the papers had evidently been dispersed to other owners too, for in 1653 Hartlib recorded that Sir Justinian Isham (of Lamport Hall, Northamptonshire, and a collector of books and manuscripts) 'hath gotten all the MS Mathematicall of Warner and . . shewed them Mr Pell'.<sup>447</sup> The papers were acquired for Isham by Charles Thynne, an acquaintance of Warner, and recently came to light among the Isham Lamport papers now in Northampton.<sup>448</sup> Most are on chemical topics, mechanics and coinage, but there is also a bundle of twenty-three sheets of mathematics, which includes copies in Warner's hand of some of Harriot's work on geometry and optics.<sup>449</sup> There is nothing, however, of Harriot's algebra.

Other Warner papers were handed over by Thorndike to Collins for safe keeping in 1667, and Collins' possession of them was confirmed in a letter to James Gregory written early in 1668:<sup>450</sup>

. . and I have some papers of Mr Warner deceased, wherein he proves if parallels be drawn to an asymptote, so as to divide the other into equal parts, the spaces between them, the hyperbola, and asymptote, are in musical progression, the which, if desired, I may communicate.

The inventory of the papers provided by Thorndike, however, reveals nothing relating to Harriot's algebra (unless contained in a bundle mysteriously entitled

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<sup>446</sup> Thorndike to Pell 23 December 1652; conversation between Pell and Thorndike recorded by Pell 17 January 1654, both in British Library Add MS 4279, ff. 275-276<sup>v</sup>. The letter of 23 December is printed in Halliwell 1841, 94.

<sup>447</sup> Clucas 1991, 45 n. 90.

<sup>448</sup> Clucas 1997. Letters 305 and 306 in the Isham Correspondence imply that something was delivered from Thynne to Isham in 1651 but are secretive about what it might have been.

<sup>449</sup> Northampton Public Records Office, IL 3422, bundle VI, ff. 1-23. All the sheets except f. 5 are in the hand of Warner. Sheets 4, 12 and 18-23 are marked 'T.H.' sheets 2, 6, 9, 14-17 are also related to Harriot's known work. In general the papers are of much the same type as those in the much larger collection of Warner's papers in British Library Add MS 4394-4396.

<sup>450</sup> Collins to Gregory early 1668, Gregory 1939, 45.

'Mr Protheroe').<sup>451</sup> Collins, for all his information on contemporary mathematical writing, knew little more of Harriot's papers than anyone else:<sup>452</sup>

The Lord Brouncker has about two sheets of Harriot de Motu et Collisione Corporum, and more of his I know not of: there is nothing of Harriot's extant but that piece that Mons. Garibal hath.<sup>453</sup>

All attempts from 1662 onwards to trace the papers met with failure. Part of the problem was that the men Harriot chose as the guardians of his mathematics outlived him by only a few years, and with their passing all first hand knowledge of the terms of the Will was lost. By 1660 the only surviving Executor was Robert Sidney, Viscount Lisle, son-in-law of Henry Percy, who was only 26 when Harriot died, but after 1632 he lived first abroad and then in semi-retirement, and no one seems to have thought of making enquiries of him. Meanwhile, both the Will and the papers became the subject of speculation and hearsay. A 1653 entry in the *Ephemerides*, for example, suggested that Sir Robert Naunton, second husband of Lower's widow, had acquired Harriot papers from Protheroe, but according to Protheroe's will, Naunton's inheritance was land, not papers, and anyway the report was suspect since it also described Protheroe as one of Harriot's last surviving friends when in fact he was the first to die after Harriot himself.<sup>454</sup> Even the Royal Society, for all its prestige and influence, failed to discover anything new: requests for surviving papers were made on behalf of the Society in 1662 to Viscount Cornbury, Earl of Clarendon and son-in-law of

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<sup>451</sup> 'An inventorie of the papers of Mr Warner', reprinted in Halliwell 1841, 95. The inventory lists 23 items, most of them on coinage or logarithmic tables. Item 18 is entitled *De resectione spatii*, a topic treated by Harriot and found more than once among Warner's papers. Item 22 is 'A bundle intituled "Mr Protheroe"'. For a detailed analysis of the inventory see Prins 1992, 25-27.

<sup>452</sup> Collins to Vernon c.1670, Shirley 1983, 9.

<sup>453</sup> Neither Monsieur Garibal nor the piece that he owned has so far been identified.

<sup>454</sup> Clucas 1991, 45. Details of Protheroe's Will in Shirley 1983, 413-414. For further speculation about Harriot's Will and papers see Collins to Vernon c.1671, Rigaud 1841, I, 153; Aubrey 1898, I, 285.

Aylesbury, and again in 1669 to John Vaughan, Earl of Carbery, brother-in-law of Protheroe, but to no avail.<sup>455</sup>

After Thorndike's death in 1672 the remaining Warner papers passed to Richard Busby, headmaster of Westminster School and, like Thorndike, a prebendary of Westminster. Busby's possession of the papers was well known, and was mentioned by both Anthony Wood and by Wallis, who wrote that Pell had 'seen and perused them'.<sup>456</sup> A final search among the Earl of Clarendon's papers was noted by Wallis and Aubrey in 1683, but Wallis was forced to conclude that: 'concerning those papers of Mr Harriot's which were supposed to be in his hands. He . . . doth assure us hath them not. So that, I guess, there are no other of them to be found.'<sup>457</sup>

The outcome of these searches is as disappointing now as it was then. Meanwhile, at Petworth, a pile of papers lay forgotten and undisturbed. They were to remain there for another hundred years until found by the German amateur astronomer Franz Xaver Zach in 1784. The discovery triggered fresh research on Harriot, and in particular the small amount of algebra among the papers selected by Zach was passed to Abraham Robertson, Oxford Savilian Professor first of Geometry and then of Astronomy in the period 1797-1826. Robertson made little of it:<sup>458</sup>

Thirteen of these [sheets] are entitled '*De numerosa potestatum res*' and contain problems similar to those in the '*Exegimus*' of the *Praxis*. Among these, several are marked as Vieta's, and this shews how necessary it is to be cautious in reasoning on what belongs to different individuals . . . The remaining

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<sup>455</sup> For a full account of the Royal Society searches see Shirley 1983, 7-9.

<sup>456</sup> Wallis to Aubrey 8 March 1684, MS Aubrey 13, f. 245. 'Thomas Harriot' in Wood 1691, I, unpaginated. After Pell's death, some of his papers too were left with Busby and were eventually deposited in the British Museum alongside those of Warner. Both sets are now classified in the modern British Library catalogue as the 'Pell collection'. Warner's papers are strangely described as 'Pell collection, second series: mathematical collection of John Pell chiefly in the hand of Warner.'

<sup>457</sup> Wallis to Aubrey, MS Aubrey 13, f. 245.

<sup>458</sup> Rigaud 1833, 52 and plate v.

three papers contain the resolution of some biquadratic equations in the way devised by Ferrari. It seemed worthwhile to have one of these lithographed. No one who may be inclined to read the whole of it can be impeded by any serious difficulty and the object for which it is now published may be evident to the most casual observer.

The ‘casual observer’ (amongst whom Robertson could perhaps be counted) could not, however, be expected to know that Harriot had here developed the *ad hoc* method devised by Ferrari to a powerful general technique and that he was the first mathematician ever to solve a biquadratic completely for all four roots, positive, negative and imaginary (earlier writers would have been satisfied with the single positive root).<sup>459</sup>

It was Stephen Peter Rigaud (1774-1839), Robertson’s successor in both Savilian chairs, who published the sheet Robertson had selected. Rigaud also made extensive and detailed notes on Harriot and his various acquaintances as well as on his papers.<sup>460</sup> He easily demolished Montucla’s claim that Harriot had very little idea of negative roots but, like Zach, his main interest was in Harriot’s astronomy. After his death, interest in Harriot’s algebra again subsided until the second half of the twentieth century.

In recent years, by far the best and most sustained research on Harriot’s science and mathematics was done by Johannes Lohne, but even he made little sense of Harriot’s treatise on equations, and in his comprehensive review of Harriot’s scientific writing, relegated it to a final section, more or less a postscript, entitled merely ‘More about the mathematics of Harriot and Viète’.<sup>461</sup> He remarked, in a footnote only, on a manuscript marked ‘(e.1) to (e.29) and sometimes (f)’ but failed to recognise it as an integral part of an extended treatise.<sup>462</sup> Torporley’s copy, which would have helped to make sense of Harriot’s

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<sup>459</sup> Girard 1629 in a section headed ‘Exemple en Stevin’ (unpaginated) criticised Stevin and Viète for not giving the full quota of roots.

<sup>460</sup> MSS Rigaud 9, 35, 56 and 61.

<sup>461</sup> Lohne 1979, 305-306.

<sup>462</sup> Lohne 1979, 305 note \*.

scattered and disordered sheets was also forgotten and has been referred to only rarely and in passing; this invaluable document has not up to now been given the attention it deserves.<sup>463</sup>

Harriot's algebra has suffered for far too long from oblivion and neglect. In the years immediately after Harriot's death, however, Warner, Cavendish and Pell all studied his work seriously. Pell, in particular, was to play a vital but hitherto unnoticed role in interpreting it to posterity.

### Wallis's account of Harriot's algebra

The contents of the *Praxis* percolated only slowly into more general use. Gibson (who had borrowed some of Warner's papers from Thorndike) was the first writer to introduce Harriot's notation and methods into a printed text but for some reason his book never became well known.<sup>464</sup> In the same year that Gibson brought out his *Syntaxis*, Wallis made his first public reference to Harriot in the Dedication of his *De sectionibus conicis*, where he introduced the theme he was to take up so strongly later:<sup>465</sup>

In the symbols used, we have followed in part our own Dr Oughtred, in part Dr Descartes (unless a better contender is the name of our own Dr Harriot, who went before Dr Descartes on almost the same path), sometimes both.

By the time Wallis came to write *A treatise of algebra* twenty years later, acrimonious quarrels with Fermat, Pascal and Dulaurens, his activities as a cryptanalyst and his own political and religious perspective had considerably hardened his attitude to the French. Wallis's account of Harriot's algebra was shot through with denigration of Descartes and accusations that he had taken ideas from Harriot without acknowledgement, criticisms which have clouded all later assessments of both Wallis and Harriot. Leaving aside, for the moment, Wallis's polemics, let us consider his description of the algebra itself.

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<sup>463</sup> There is a footnote reference to Torporley's copy in Tanner 1980, 137 n. 20. The Torporley manuscripts were also mentioned in an entirely different connection in Seaton 1956, 111-114.

<sup>464</sup> Gibson 1655, see thesis Chapter 4.

<sup>465</sup> Wallis 1655a, 'Dedicatio'; Wallis 1695, 294, translation JS.

Wallis began, after a few brief biographical details on Harriot, with a summary of the material covered in the Definitions and Section 1 of the *Praxis*, particularly noting Harriot's use of lower case letters, and the fact that his notation was unambiguous, clear and free from geometrical considerations.<sup>466</sup> Wallis's next chapter opened promisingly enough:<sup>467</sup>

Beside those conveniences in the Notation mentioned in the former Chapter, (which are things less considerable:) Mr Harriot, as to the *Nature of Equations*, (wherein lyes the main Mystery of *Algebra*;) hath made much more improvement. Discovering the true Rise of Compound [polynomial] Equations; and Reducing them to the Originals from whence they arise. Which he enters upon in his Second Section.

Then Wallis made the first of the comments which has brought his account into such disrepute:

And here first, Beside the *Positive* or *Affirmative* Roots, (which he doth, through his whole Treatise, more especially pursue, as the principal and most considerable:) He takes in also the *Negative* or *Privative* Roots; which by some are neglected.

At first this seems a very strange remark, for nowhere in the *Praxis* do negative roots appear.<sup>468</sup> Their absence was so obvious that one reader after another was forced to question Wallis's credibility. But in his parentheses Wallis mentioned something else: a *Treatise*, in which Harriot particularly pursued positive roots. This is an accurate description of Harriot's *unpublished* treatise where, as we have seen, and just as Wallis described, Harriot did concentrate on positive roots, introducing negative roots only at a later stage. There is already a strong hint here that Wallis was working not just from the published text but from Harriot's

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<sup>466</sup> *Treatise of algebra*, 125-126.

<sup>467</sup> *Treatise of algebra*, 128.

<sup>468</sup> Negative substitutions were occasionally used later in the book, see *Praxis*, 97, but this was not what Wallis meant here.

unpublished papers. Wallis's use of 'here' was not only vague but confusing: he was ascribing to the *Praxis* something which was evident only in the manuscripts.

A few pages later there was a similarly misleading remark about imaginary roots:<sup>469</sup>

And of such imaginary Roots, we find Mr Harriot particularly to take notice (in the Solution of Cubick Equations) in his 13<sup>th</sup> Example of his 6<sup>th</sup> Section; pag. 100.

What we actually find on page 100, is a statement that the equation  $aaa - 3bba = 2ccc$ , is 'impossible' because it requires as part of its solution the term  $\sqrt{-ddddd}$ , which is 'inexplicable'. In other words, the writer of the *Praxis* 'particularly took notice' of such roots only to dismiss them as rapidly as possible. There is no other reference to imaginary roots in the *Praxis*. Harriot's manuscripts, on the other hand, supply ample evidence that he himself worked quite comfortably with imaginary solutions. It is impossible to avoid the conclusion that Wallis was not making it at all clear where his information came from. This was so uncharacteristic of Wallis, who was well used to public argument, and to the accurate quotation of chapter, verse and line number, that one is forced to ask whether he was deliberately throwing a smoke-screen around his true sources.

Wallis's specific examples of Harriot's method confirm the suspicion that he was working from more than the printed text. The first example he gave was the following:<sup>470</sup>

$$\begin{array}{rcl}
 a = +b & a - b = 0 & \\
 a = -c & a + c = 0 & \\
 & \hline
 & aa - ba & \\
 & + ca - bc & \\
 & \hline
 & aa - ba + ca - bc = 00 & 
 \end{array}$$

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<sup>469</sup> *Treatise of algebra*, 134.

<sup>470</sup> *Treatise of algebra*, 129.

And then Adding or Subducting  $bc$  to each side [Harriot] deduceth such as these,  $aa - ba + ca = bc$ : Which he calls *Canonical Equations*. (3)

There are two important features which identify this example as being from the manuscript version of Harriot's treatise: first, Wallis has used the substitution  $a = -c$  which never appeared in the *Praxis*, but which seems to have been tried (though crossed out) in the manuscript; second, Wallis used the notation  $00$  for two zeros multiplied together, a notation required by the principles of homogeneity, and often used by Harriot, but which never appeared in the *Praxis*. In short, this example is very much like the corresponding example in Harriot's manuscript sheet d.1) as may be seen by comparing (3) with the first paragraph of Harriot's (1) above. Furthermore, Wallis went on, just as Harriot did, to show that if  $a = b$ , then the canonical equation reduces to  $bb - bb + cb - bc = 0$ , as required. The correspondence between Wallis's version and Harriot's is so close that it is impossible not to suspect that Wallis actually had sight or possession of a copy of Harriot's work

Evidence that the latter was indeed the case comes from a letter Wallis wrote to Samuel Morland in 1689, in which he compared Harriot's and Descartes' work in some detail.<sup>471</sup> As examples of Harriot's use of negative and imaginary roots, Wallis gave  $a = -f$  and  $a = \pm\sqrt{-df}$ , and page numbers of the *Praxis* where the relevant equations appeared. But once again his citations were less than transparent: in the *Praxis* the equations were shorn of their negative or imaginary roots which were to be found only in the manuscripts, in sheets d.7.2<sup>0</sup>) and d.13.2.<sup>0</sup>) (see (2) above).

How did Wallis come by his knowledge of the manuscripts, given the disappearance of Harriot's originals? His earliest opportunity would have been around 1650 when he shared a study with Ward, but his letter to Aubrey in 1683 implied that he had no clear recollection of having seen the papers at that period. Wallis may have been unwilling to share all he knew with Aubrey, and it is just possible that he saw and copied some of Harriot's papers in the early 1650s. He

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<sup>471</sup> Wallis to Morland 12 March 1689, Wallis 1693, 209-210.



had a much more immediate source, however, in John Pell. Wallis never mentioned Pell in connection with Harriot in *A treatise of algebra*, but some years after Pell's death in 1685 he was provoked into being more explicit by dismissive remarks made by Baillet in his 1691 *La vie de monsieur Des-Cartes*.<sup>472</sup> Wallis responded in a piece headed *De Harrioto addenda*. It contains the following very important but previously unremarked paragraph:<sup>473</sup>

*Certe nemo omnium judicaverit, haec ante ab Harrioto non fuisse tradita. Quid Pellius ipse senserit, ego aliquatenus intelligo; ut qui me hac de re saepius compellavit; & ex cujus ore descripsi quod hac de re dixi; eique postquam erat descriptum, ostendi, (examinandum, immutandum, emendandum pro arbitrio suo, siquid alias dictum malit) antequam prelo subiceretur, totumque illud quod inde prodiit, assentiente & approbante Pellio dictum est.*

Certainly, no one at all has judged any of this to have been taught before Harriot. Which Pell himself would have known at first hand, and I some time later came to realise; as a result of which, Pell urged me all the more often in this; and from his words I have written everything I have said; and afterward I showed him what I wrote (to be examined, altered, corrected as he decided, or preferred) before it went to press, and everything which was published was said with Pell's assent and approval.

Such close collaboration between Pell and Wallis adds a new dimension to the story. In particular it sheds light on Wallis's carping criticism of Descartes: Pell had known Descartes personally and, unlike Wallis, had some reason to dislike him. His antagonism seems to have arisen at their first meeting in 1646, when Descartes apparently discouraged Pell from his efforts to edit Diophantus.<sup>474</sup> Later, Pell appears to have been disgruntled at Descartes' reception of his *Idea* (his plan to compile a catalogue of mathematics and mathematicians, and a library

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<sup>472</sup> Baillet 1691, Book VIII, 541.

<sup>473</sup> From the opening paragraph of *De Harrioto addenda*, which follows the preface (unpaginated) of Wallis 1693.

<sup>474</sup> Jacquot 1952b, 183.

of their most important works).<sup>475</sup> Collins, with whom Pell lodged for a time, knew something of this affair, for he reported it to Leibniz:<sup>476</sup>

The said Doctor [Pell], being censorious of others, and incommunicative, himself declining discourses about his methods, was at last censured by Des Cartes for those assertions, concerning whom the Doctor never had any extraordinary esteem. And those letters or censures of Des Cartes, one Mr Haak, . . . hath a copy of, but, upon the account of his friendship to Dr Pell will not impart.

Haak was finally persuaded to publish the letters in the *Philosophical collections* in 1682, and it turned out that Descartes' attitude was not so much one of antagonism as of indifference ('I inspected the *Mathematical idea* only incidentally and now only recollect that there was nothing with which I greatly disagreed.')<sup>477</sup> but to Pell it probably amounted to much the same thing.

The above letter was undated, but Collins again complained about Pell's difficult and uncommunicative nature, to John Beale in 1672:<sup>478</sup>

Dr Pell hath made notes, as I have heard him affirm, on that author [Pappus]; but to incite him to publish any thing seems to be as vain an endeavour, as to think of grasping the Italian Alps, in order to their removal. He hath been a man accounted incommunicable; the [Royal] Society (not to mention myself) have found him so: had they not they might have recommended him to a pension from his Majesty of France.

Pell's reluctance to commit himself in print was further evident in connection with the *Teutsche Algebra* of his pupil, Johann Rahn. Pell probably wrote much of the material for the original edition and certainly added greatly to the English translation but his name appeared in the latter only in the obscure phrase 'Altered

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<sup>475</sup> Pell 1650.

<sup>476</sup> Collins to Oldenburg for Leibniz, undated, Rigaud 1841, I, 247.

<sup>477</sup> Pell 1682 and Descartes 1682. For translations of both pieces see Fauvel and Gray 1987, 310-314.

<sup>478</sup> Collins to Beale 20 August 1672, Rigaud 1841, I, 196-197.

and Augmented by D. P.’<sup>479</sup> It would seem that Pell’s desire for anonymity led him to insist on keeping his name out of Wallis’s work too. If so, he did inestimable harm, for he forced Wallis to blur the boundaries between his manuscript and printed sources, and brought the whole veracity of the account into question.

Wallis’s participation in a late search for Harriot’s papers in 1683-84 does not contradict the claim that he already had access through Pell to Harriot’s algebraic treatise: the search was for Harriot’s work in general, and for originals, not copies. In *A treatise of algebra*, Wallis remarked only that the disappearance of Harriot’s *other* treatises prevented him from detailing any further applications of his algebra:<sup>480</sup>

What uses [Harriot] hath made of Algebra in order to other parts of mathematical knowledge, in his other Treatises, I cannot say; because they are not publick: nor do I know in whose hands they are; if extant; nor whether they are ever like to see the light.

In March 1677, however, having recently completed the first draft of *A treatise of algebra* Wallis wrote a long preamble in the Savile Library copy of the *Praxis*, in which he stated quite clearly that he had seen some of Harriot’s unpublished work:<sup>481</sup>

There were many other very worthy pieces of Mr Harriots doing, left behind him, & well worth the publishing: as appears by Mr Warners Preface, & Title-page & some of them I have seen. But in who’s hands they now are, or whether they be since perished, I cannot tell.

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<sup>479</sup> Rahn 1659 and Rahn-Pell 1668. A draft of the 1668 title page in Pell’s hand, exactly as it was eventually printed, is in British Library Add MS 4414, f. 2. For further discussion of Pell’s attitude to publication see. Scriba 1974 and thesis Chapter 6.

<sup>480</sup> *Treatise of algebra*, 198.

<sup>481</sup> The full text is reprinted in Shirley 1983, 10-11, but the shelfmark is incorrectly quoted. The copy used and annotated by Wallis was originally owned by Charles Cavendish, and given by him to Robert Payne. After Payne’s death the book was acquired for the Savile Library where it is now Savile O.9.

In 1683 he wrote to Aubrey:<sup>482</sup>

I have never read any of his things, but that only of his Algebra; which hath had the good hap to be published by Mr W.W., as a prodromus to some other of his works; which at the same time, he gave hope of publishing, but hath not done it.

At first sight, this statement appears to contradict his earlier claim to have seen some of Harriot's 'pieces' in manuscript, but there is a distinction to be made between having seen something and having read it. There may also have been a difference between what Wallis would write in a private library book and what he would wish to convey to Aubrey. It is possible, however, to interpret the 1683 passage further. The text can be read as: 'I have never read anything of his work [in manuscript] except his algebra. This has fortunately been published by Mr Warner as a forerunner of some further works.' In other words, Wallis was implying, as he had elsewhere, that the algebraic content of the manuscripts was much the same as the content of the *Praxis*. The major problem with his entire account was precisely that he failed to draw a clear distinction between the two.

All the evidence suggests that Wallis had direct access during the 1670s and 1680s to a copy of Harriot's algebraic treatise,<sup>483</sup> and with his own acute mathematical insight, and his keen interest in the history of his subject, he developed a better idea of Harriot's work than any of his contemporaries, except perhaps Pell himself. His grasp of Harriot's work as a whole, and of its deeper implications, was evident throughout his account. He devoted three chapters following his first example, (3), to showing how quadratics, cubics and biquadratics could be built up by multiplication, and gave careful cross-references to the relevant examples in the *Praxis* wherever he could, but he also included those equations with only negative roots which Warner had so conspicuously left out.<sup>484</sup> Not only this, but Wallis took some pains to point out, in four further

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<sup>482</sup> Wallis to Aubrey 20 July 1683, MS Aubrey 13, f. 242.

<sup>483</sup> Wallis's detailed reference to Harriot's treatise in his 1689 letter to Morland implies that a copy was still in his possession four years after Pell's death. This raises questions as yet unanswered about the subsequent fate of Wallis's copy.

<sup>484</sup> *Treatise of algebra*, chapters 32-34.

chapters, features that he considered obvious from Harriot's work, but which had never been explicitly stated: the clear relationships between the coefficients and the roots of a polynomial equation, for instance, or the fact that the degree of such an equation could be decreased by division, just as it could be increased by multiplication.<sup>485</sup> Generally Wallis was quite clear that these results were not explicitly stated in the *Praxis* but could be inferred from its contents, as the following quotations show (my italics):<sup>486</sup>

*Whence it follows, That all Cubick Equations have (Real or Imaginary) Three Roots, (all Affirmative or all Negative or partly the one, partly the other.) . .*

*'Tis manifest also, that as Compound Equations are made up of others more simple, by Multiplication; So they may by like Divisions, be reduced into those Simples again. . .*

*'Tis also made manifest (from these Compositions,) not only, how many Roots (Real or Imaginary,) every Equation contains, (viz. so many as there are the Dimensions of the Highest Term:) But Likewise, of what Members each of the Coefficients are made up. Which appears, without further trouble, by a bare inspection of the Composition. . .*

*And whereas Mr Harriot gives Rules to determine how many Affirmative Roots there are in any Equation proposed; the same Rules (by this means) serve as well to determine, how many Negatives are therein real. . .*

*And these [improvements] are either explicitly delivered by him, in express words; or be obvious Remarks, upon the bare inspection of what he delivers. . .*

*How he [Descartes] came by that Rule, he doth no where tell us; nor give us any Demonstration of it. . . But from Harriot's Principles, It follows naturally.*

This approach was typical of Wallis's account: over nineteen chapters he described or quoted the entire content of the *Praxis*, much of it verbatim, but

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<sup>485</sup> *Treatise of algebra*, chapters 35-38.

<sup>486</sup> *Treatise of algebra*, 135, 141, 142, 144, 200, 208-209 respectively.

repeatedly filled in what was missing, or what he considered the obvious consequences of Harriot's work. Thus at, Problems 19, 20 and 21 of Section 3, where Torporley had stumbled and Warner had fallen, Wallis restored the missing conditions without difficulty, and wrote them into his copy of the *Praxis*. I have found only one claim, hardly a serious one, which can not be substantiated from the surviving manuscripts: that Harriot proved the inequality involving twelfth powers which appeared in the middle of Proposition 6, Section 5.<sup>487</sup>

It was wrongly assumed by Newton and Leibniz, and by others afterwards, that Wallis attributed to Harriot the 'Rule of Signs', a rule later formulated by Descartes which determined the number of positive roots of a polynomial from the way the signs of the coefficients alternated. What Wallis actually said was that the Rule of Signs only held when all the roots were real, and that Descartes might have realised this if he had read Harriot more carefully.<sup>488</sup> Harriot's work, said Wallis, enabled one to determine just how many roots were real, and it was *this* procedure that Wallis referred to as 'Harriot's rule'. It was never explicitly stated as such by Harriot, but was yet another interpretation by Wallis of what Harriot's work implied. The true origin of the Rule of Signs, like so much else in European algebra, could in fact be traced back to the pioneering work of Cardano, in which Wallis had never taken any great interest.

With regard to Wallis's other complaints about Descartes, a single example will serve to illustrate the many that are scattered through Wallis's account:<sup>489</sup>

Mr Harriot . . doth in divers things vary from the Method of Vieta and Oughtred. And hath made very many advantageous improvements in this Art; and hath laid the foundation on which Des Cartes (though without naming him,) hath built the greatest part (if not the whole) of his Algebra or Geometry. Without which, that whole Superstructure of Des Cartes (I doubt) had never been.

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<sup>487</sup> *Treatise of algebra*, 158; *Praxis*, 86. The proof of this inequality would not have been beyond Harriot but would have been a severe test of his notation.

<sup>488</sup> *Treatise of algebra*, 158-159.

<sup>489</sup> *Treatise of algebra*, 126.

Was this Pell speaking through Wallis? If so, he did neither Wallis nor Harriot a service, for such remarks laid Wallis open to scorn. Descartes may well have seen the *Praxis* when it was published (he visited England in 1631), or he may, through Cavendish, have been aware of Harriot's work even earlier. Perhaps it is true that his ideas on factorising polynomials were to some extent developed from what he had seen or heard of Harriot. If so, it would indeed have been courteous to say so, but by 1637 Descartes was engaged on a grand plan of his own. Even the most superficial comparison of Descartes' work with Harriot's reveals significant differences of style and intent: Harriot's treatise was an exposition of pure algebra whereas Descartes in *La géométrie* passed rapidly over the essentials of algebra in order to apply them to geometry, which, as the title suggested, was the book's main concern. Both men were deeply indebted to Viète,<sup>490</sup> but Harriot's outstanding contribution to algebra was to develop Viète's work on equations as a study in its own right, whereas Descartes took up Viète's ideas of using algebra for the sake of geometry. If Wallis had taken Viète's work more seriously, he would have recognised it as the foundation of the 'advantageous improvements' made by both Harriot and Descartes.

Wallis's criticism of Descartes proved unacceptable to his own contemporaries and he moderated his remarks a little when *A treatise of algebra* was translated into Latin for his collected works in 1693. In the new preface and *De Harrioto addenda* he was careful not to charge Descartes with plagiarism, but he continued to insist, correctly, that Harriot's ideas had preceded Descartes' by twenty, or even forty, years. Following the chapters devoted to Harriot he published his 1689 correspondence with Morland in which he actually denied calling Descartes a plagiarist, but the text of the chapters themselves remained essentially unchanged. For our present purposes it is best to set Wallis's diatribe against Descartes aside, and consider instead what he was trying to do for Harriot.

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<sup>490</sup> Descartes claimed that he had never read Viète. This may have been so, but by the time he wrote *La géométrie*, Viète's ideas had been in circulation for almost half a century and Descartes can hardly have been unaware of them.

Wallis saw as clearly as Pell the true magnitude of what Harriot had done, and was concerned, with Pell's encouragement, to put the record straight. His description of Harriot's work was intended not just as straight rendering of content but as an assessment of the work in the overall history of algebra, and its potential for development. In this he can only be said to have succeeded. Everything he said can be substantiated from careful reading of the *Praxis* or the manuscripts, and his exposition of what could be built on Harriot's foundation was essentially correct. Wallis's final list of twenty-five 'Improvements of Algebra to be found in Harriot' was a fair summary of what could be found in Harriot's work or easily deduced from it.<sup>491</sup> His treatment still stands as the most thorough and detailed analysis of Harriot's algebra to date, and if he had not been so aggressive towards Descartes, or so secretive about his sources, his account might have stood as the fine testament to Harriot that he intended it to be.

### **Harriot's contribution to algebra**

What should we now consider to be the 'Improvements of Algebra to be found in Harriot'? The first and most obvious must be his notation: the use of lower case letters, with repetition to indicate multiplication, freed algebra for the first time from the geometrical connotations it had always previously carried. The only significant difference between modern notation and Harriot's is in the use of indices: terms such as *aaaabb* seemed to beg for them, and why Harriot never introduced them is a mystery. Torporley in copying Harriot's manuscripts did occasionally introduce index notation in the form  $a^I$ ,  $a^{II}$ ,  $a^{III}$ ,  $a^{IV}$ , and clearly the use of such abbreviations was only a matter of time.<sup>492</sup> The lack in Harriot, however, though tedious for the writer, did nothing to hinder the reader, and Harriot's work has a beauty and lucidity which have often been noted and which make it a pleasure to read even today. Oughtred later took great pains to devise an algebraic notation which 'plainly presenteth to the eye the whole course and

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<sup>491</sup> *Treatise of algebra*, 199-200.

<sup>492</sup> Sion College MS Arc L.40.2/L.40, f. 43.



processe of every operation and argumentation', but never succeeded to the extent that Harriot had done more than twenty years earlier.

Dispensing with geometrical baggage, however, led to more than just the simplification of notation: it also made possible Harriot's second great achievement, the handling of equations at a purely symbolic level. If the achievement of Descartes was to show how algebra could be applied to geometry, the achievement of Harriot was to liberate algebra from geometry altogether, so that for the first time it could become truly a subject in its own right. Of course, Harriot made use of algebra in geometrical applications; in addition to the problems from Viète already mentioned, he also transcribed the fourteen propositions of Euclid II into algebra,<sup>493</sup> something Oughtred much later regarded as the crowning achievement of his *Clavis*. Harriot's finest contribution, however, was 'to treat of Algebra purely by itself, and from its own principles, without dependance on Geometry, or any connexion therewith.'<sup>494</sup> Harriot's interest in the essential structure of equations was firmly in the tradition of Cardano, Bombelli and Viète, and within this tradition, Harriot should be seen as the first to dispense entirely with geometric considerations, and as the first forerunner of modern abstract algebra.

Harriot's third outstanding achievement was his crucial insight into the way polynomials could be built up as products of linear or quadratic factors and to see that such composition was in turn a powerful analytic tool. In Harriot's crystal clear layout, results about the number and kind of roots, and the relations between the roots and the coefficients became immediately obvious. Wallis pointed this out repeatedly but modern readers of the *Praxis*, too often distracted by the subject of negative roots, have barely remarked upon it.<sup>495</sup> It was, however,

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<sup>493</sup> British Library Add MS 6785, ff. 153-156 (true order reversed).

<sup>494</sup> *Treatise of algebra*, 198.

<sup>495</sup> Cajori 1928. Pycior 1997, 54-64; 57, 64 recognised Harriot's separation of algebra from geometry but dismissed the *Praxis* as 'little more than a basic introduction to an equation theory that recognized only positive real roots.'

evident to Hutton who, a century after Wallis, said the same thing in more measured tones:<sup>496</sup>

[Harriot] shewed the universal generation of all the compound or affected equations, by the continual multiplication of so many simple ones, or binomial roots; thereby plainly exhibiting to the eye the whole circumstances of the nature, mystery and number of the roots of equations; with the composition and relations of the coefficients of the terms; and from which many of the most important properties have since been deduced.

Harriot was not in the habit of describing his mathematics verbally, but if he failed to state such results explicitly it did not mean he was not aware of them: his entire work on equations was based on the way coefficients were composed from roots. Such relationships, between roots and coefficients, were to become the foundation of all subsequent work on polynomial equations, and were to lead eventually to the development of modern abstract algebra.<sup>497</sup>

These three achievements alone: notation, the liberation from geometry, and the composition of polynomials, were enough to place Harriot among the first rank of early algebraists, alongside Cardano, Viète and Descartes. For his time, he was, as Pell so rightly said, ‘so learned, that had he published all he knew in algebra, he would have left little of the chief mysteries of that art unhandled.’<sup>498</sup> The tragedy was, of course, that Harriot himself published nothing, and those who did so failed to do him justice. Instead, from 1637 onwards, Descartes’ *La géométrie* became the foundation and inspiration for the next generation of continental mathematicians. For a time, Harriot’s influence was evident in the published work of English algebraists,<sup>499</sup> but eventually Descartes’ work became

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<sup>496</sup> Hutton 1796, 91; Hutton 1812, 286.

<sup>497</sup> Van der Waerden 1985, 76-88, shows how modern abstract algebra grew from considerations of relationships between roots and coefficients of polynomials, but Harriot’s work is not mentioned.

<sup>498</sup> Collins to Vernon c.1671, Rigaud 1841, I, 153.

<sup>499</sup> Harriot’s notation appears in Moore 1650; Gibson 1655; Dary 1664; Kersey 1673-74; Leybourn 1690; Anderson 1693.

the dominant influence in England too. No wonder that Wallis, already deeply suspicious of the French, became bitter as he came to know Harriot's algebra better at what he, like Pell, saw as Descartes' usurpation of Harriot's rightful place.

Wallis, like Torporley and Warner before him, did his best to ensure that Harriot was given the recognition he deserved, but later readers of Wallis's account, lacking the supporting evidence of the manuscripts, saw only his apparent exaggeration and polemic, and dismissed it. Harriot's procrastination had cost him dear, and those who came after him failed, through incompetence, incomprehension or misjudgement, ever to reward him with the 'Gharland of the great Invention of Algebra'.



## Chapter 6

### Moving the Alps: uncovering the life and mathematics of John Pell

#### Summary

John Pell (1611-1685) was respected as a mathematician throughout his life but rarely agreed to publish or share his work. He left large quantities of mathematical papers, now in the British Library, which remain unsorted and under-researched. Examination of the papers reveals that Pell discussed mathematics with Wallis over many years, and that a significant part of Wallis's *Treatise of algebra* contains work initiated or influenced by Pell. The earliest contact between the two, however, came about unknown to either, when Wallis deciphered letters written by Pell from the Netherlands in 1650. Pell's authorship of the letters, now in the Bodleian Library, has not previously been recognised, and they shed new light on his political activities during the interregnum.

Following his long accounts of the algebra of Oughtred and Harriot, Wallis gave a much briefer description of the work of another English mathematician, John Pell (1611-1685). In two short chapters (57 and 59) of *A treatise of algebra* Wallis quoted from Pell's only significant published work, the 1668 *Introduction to algebra*,<sup>500</sup> and drew attention to Pell's treatment of indeterminate equations and his idiosyncratic notation and layout (to be discussed below). Then, it seemed, Wallis wandered off into other topics. Apart from the two chapters specifically devoted to Pell, *A treatise of algebra* from Chapter 55 (immediately following the section on Harriot) to Chapter 72 (preceding Wallis's account of his *Arithmetica infinitorum*) is filled with miscellaneous topics of varying relevance to the history of algebra and with little

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<sup>500</sup> The *Introduction to algebra* began as a translation by Thomas Brancker of the *Teutsche Algebra* of Pell's student, Johannes Rahn. Pell revised and added to the English translation which will therefore be referred to in this chapter as the Rahn-Pell *Algebra*.

relation to each other. These eighteen chapters read like an anthology of assorted material that Wallis for one reason or another thought fit to include. The chapters share a common secret, however: at least fourteen of them contain material which can be traced to Pell, the remaining four being no more than links, summaries or afterthoughts. There are also two important earlier chapters (10 and 11) whose contents appear to have originated with Pell, as well as the section on Harriot already discussed. The material in which Pell had some influence therefore constitutes well over a third of *A treatise of algebra*. This has never before been suspected and is one of the most surprising discoveries to emerge from the present study.

### **The incommunicable Doctor Pell**

That Pell's role has remained so long undiscovered is typical of our knowledge of him, for he is one of the most enigmatic and under-researched of the seventeenth-century mathematicians. He was well regarded by his contemporaries: William Brouncker, for instance, President of the Royal Society, nominated him as his Deputy in 1676.<sup>501</sup> Pell, however, published very little to justify his reputation. John Collins, who knew him well, wrote:<sup>502</sup>

As to his knowledge, I take him to be a very learned man. More knowing in algebra, in some respects, than any other . .

but that:

To incite him to publish anything seems to be as vain an endeavour, as to think of grasping the Italian Alps, in order to their removal. He hath been a man accounted incommunicable; the [Royal] Society (not to mention myself) have found him so.

Collins was correct about Pell's learning, for Pell was an avid reader of mathematical texts. He made notes on the work of almost every sixteenth- or

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<sup>501</sup> The original nomination, signed by Brouncker, is preserved in British Library Add MS 4423, f. 237.

<sup>502</sup> Collins to Beale 20 August 1672; Collins for Leibniz, undated, Rigaud 1841, I, 196-197, 247.

seventeenth-century English or continental mathematician of note: Cardano, Nuñez, Ramus, Bombelli, Stevin, Viète, Harriot, Oughtred, Briggs, Gellibrand, Hérigone, Bachet, Descartes, Fermat, Girard, Gibson, Barrow, Mercator, Huygens, Billy, Frenicle, Stampioen, Dary, Baker and others. His surviving papers present the researcher with a daunting task: thirty-three large volumes in the British Library<sup>503</sup> contain numerous tables, calculations, worked problems, notes on books read, occasional letters and old envelopes (used for more calculations) with a few sermon notes and theological speculations interspersed. He wrote in a small neat hand and often used red ink (now faded) to highlight parts of his text. His correspondence with Cavendish, Collins, Hartlib, Mersenne, Morland and others fills three further volumes.<sup>504</sup> The mathematical papers are chronologically and thematically in disarray: they range over Pell's entire working life from 1627 to 1684, but many are undated, and all are in confusion. Preliminary examination, however, has revealed a number of items which shed light not only on some of Pell's mathematical interests but also on his relationship with Wallis.

In 1627 Pell was sixteen and a student at Trinity College, Cambridge. An item dated August 1628 is a booklet of multiplication tables up to  $9 \times 100$  with instructions for use,<sup>505</sup> the earliest evidence of Pell's lifelong interest in table-making. In the same year Pell wrote to Henry Briggs (who replied as 'Yr very lovinge frende, Henrie Briggs'<sup>506</sup>) with queries about antilogarithms and the interpolation of tables of sines. Pell produced a number of slim handwritten pamphlets on mathematical topics over the next five or six years,<sup>507</sup> and they reveal something that was to be characteristic of him all his life: a curious

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<sup>503</sup> British Library Add MS 4397-4404, 4407-4431.

<sup>504</sup> British Library Add MS 4278-4280.

<sup>505</sup> British Library Add MS 4397, f. 1.

<sup>506</sup> Briggs to Pell 25 October 1628, British Library Add MS 4398, f. 137, printed in Halliwell 1841, 55-57.

<sup>507</sup> There are a number of these small booklets written by Pell in, for example, British Library Add MS 4431.

reluctance to reveal his name. Almost all the early booklets have a carefully designed title page with date, but no author. One, for instance, is entitled *Linea proportionata* and in the neatly ruled space where the author should appear are the words *By A. FALE, Gent.*<sup>508</sup> Another copy of this simply has the word *By* followed by a blank space, as though Pell was uncertain what pseudonym to choose.<sup>509</sup> This same reluctance to reveal himself was evident in his *Idea* of mathematics written before 1630 while he was still a student, in which he suggested the setting up of an index of mathematicians and mathematical texts with an accompanying library of books and instruments. The *Idea* circulated in manuscript as soon as it was written and was first printed in 1638, but only anonymously.<sup>510</sup>

During the 1630s Pell taught in Sussex, and married Ithamaria Reginalds with whom he eventually had eight children. Later he moved to London and was connected with Samuel Hartlib and John Dury and their aim of promoting a Protestant commonwealth of learning through the collection and free exchange of knowledge, and a 'Christian Association whereof all the Members might be serviceable to each other, and to the Publicke'.<sup>511</sup> (It was Dury who published Pell's *Idea* in 1650.) Through his friendship with Walter Warner, the editor of Harriot's papers, Pell also became acquainted with Thomas Aylesbury and Charles Cavendish.<sup>512</sup>

In December 1643 Pell left England for Amsterdam, 'not bringing any of my bookes or papers with me: nor hardly clothes, for . . . I then thought not to stay heere above a fortnight.'<sup>513</sup> The reason for this ill-prepared journey is not known

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<sup>508</sup> British Library Add MS 4431, f. 73.

<sup>509</sup> British Library Add MS 4431, f. 70.

<sup>510</sup> I have not seen the 1638 edition of the *Idea* but Pell referred to it in British Library Add MS 4399, f. 1. The *Idea* was reprinted in Dury 1650, Pell 1682 and Fauvel and Gray 1987, 310-313.

<sup>511</sup> Dury 1651, Preface. See also Clucas 1991.

<sup>512</sup> Jacquot 1952a, 27 and 1952b, 175-176, 183-184. For Pell's correspondence with Cavendish see Halliwell 1841, 72-78.

<sup>513</sup> Pell to Cavendish 7 August 1644, Halliwell 1841, 80-81.



but, once in Amsterdam, Pell was offered a teaching post and held it for over two years. In 1646 he moved to the new *Collegium Auriacum* (the Orange College or Illustre School) in Breda<sup>514</sup> and remained there until 1652. During his years in the Netherlands he expended much time and energy on a refutation of the quadrature of Longomontanus,<sup>515</sup> but did little else to further his mathematical reputation. Huygens, who was a student at Breda from 1647 to 1649, later remarked that he had learned nothing from Pell.<sup>516</sup>

Newly discovered evidence shows that Pell during his Breda years was concerned with more than mathematics. On 30 January 1649 Charles I was beheaded in London, an event abhorrent to many (including Wallis) who had loyally supported the Parliamentary cause. The king's eldest son, Charles Stuart, then in Breda, was immediately proclaimed Charles II in Scotland and Ireland, but the Scots demanded in return full acceptance of Presbyterianism. The Scottish commissioners sent to Breda to negotiate terms were accompanied by three representatives of the English Presbyterians, one of them Captain Silas Titus, who had served Charles I in the last months of his captivity on the Isle of Wight. As the political manoeuvring stalled, Titus, stranded in Breda and with an interest in mathematics, became friends with Pell. Then in 1650 a number of partly coded letters were sent from Breda to London and some of them mentioned Titus. Sixteen of the letters were intercepted, and deciphered in England by none other than Wallis, who in 1653 placed copies in the Bodleian Library in Oxford.<sup>517</sup> Nine of the sixteen letters are signed J.P. but the writer has not previously been recognised as John Pell. There is no hint that Wallis recognised Pell's identity, and indeed no reason why he should have done so. The letters deserve closer scrutiny, which I hope in future to give, and what follows here is no more than a preliminary description of this important new find.

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<sup>514</sup> Sassen 1966; Lindeboom 1971.

<sup>515</sup> Van Maanen 1986.

<sup>516</sup> Van Maanen 1986, 345.

<sup>517</sup> MS Eng. misc. e. 475, ff. 144-206, copied again Wallis's hand in MS e Musaeo 203, ff. 134-201.

The sixteen letters were all written between April and June 1650, in a mixture of cipher and plain text (in the extracts below the text enciphered in the originals is indicated by underlining). The codes vary in detail only and use numbers substituted for letters, pairs of letters, or common words. The plain text itself often carries hidden meaning: 'advise w<sup>t</sup> you think of that commodity as y<sup>e</sup> times now rule, whether sale may be made or not, and at what price' (6 April) or 'the twelve bookes you desired be ready for the first winde, directed as you gave order' (13 May). The 'commodity', frequently referred to, was Charles Stuart. In keeping with this disguise, the letters were usually addressed as to London merchants: Mr Jacob van Delph, S<sup>r</sup> Peter van der Willigen etc. The writers eventually realised that letters were being intercepted, but one, who signed himself J.C., claimed to be confident in his ciphers:<sup>518</sup> 'were they possible to be deciphered (*though I hardly beleeeve there is a man in the world can doe it*) . .' He did not know Wallis.

Pell's letters report detailed information concerning the negotiations to friends in London, and he sought their advice as to how he personally should proceed:<sup>519</sup>

Mr the king wil be at Hage and speedily goe for Scotland so y<sup>t</sup> I shall desire your advise [sic] and our friend's advice what to doe whether I should goe with the king or not and what good may be done at Portsmouth etc for I am very much courtedd by the king and his party to goe with him for Scotland. And I think y<sup>t</sup> Mr the Scotch commisioners will desire it likewise.

Interception of this and other letters meant that no instructions ever came, and in June 1650 Charles Stuart sailed for Scotland without the pleasure of Pell's company. His defeat at Dunbar three months later and at Worcester in 1651 put an end for the time being to any further hopes of a Royalist recovery.

Titus was refused permission to re-enter England but Pell returned in 1652. Ever short of money, he perhaps decided to put his skills to new service, and in

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<sup>518</sup> MS Eng.misc. e. 475, ff. 167-168, my italics.

<sup>519</sup> MS Eng.misc. e. 475, f. 216.

1654 was posted to Zurich as Cromwell's agent, entrusted with the task of drawing the Protestant cantons into closer relationship with England. This period of Pell's life is better documented than any other since his letters to John Thurloe, Cromwell's head of intelligence and to Samuel Morland, another agent, have been published along with Pell's diary of his Zurich years.<sup>520</sup> It may be noted that Pell addressed Thurloe as 'Mr Adrian Peters, merchant at London', the kind of cover he had also used in Breda. In those days Pell had described Cromwell and his allies as 'those villains now in power'.<sup>521</sup> Now, exactly four years later, he wrote: 'though he [Cromwell] employ many factors abroad more exercised than I have been, yet I hope he shall find none more careful and faithful'.<sup>522</sup> Pell returned to London three weeks before Cromwell's death in 1658 and what he did in the next two years is not known, but he suffered no fall from favour after Charles II was proclaimed king in London in 1660. On the contrary, he was rewarded with a living (in Fobbing, Essex) from the king and another (Laindon, Essex) in 1663 from Gilbert Sheldon who, like Titus, had attended Charles I on the Isle-of-Wight and had supported Charles II in exile.

It is almost impossible to say where Pell's true allegiance lay during the 1640s and 1650s. In the early years of the Civil War he was intimate not only with the Hartlib circle but also with men like Thomas Aylesbury and Charles Cavendish who were prominent Royalists. The true reason for his move to Holland in 1643 is unknown: he later told Aubrey he went to take up a teaching post,<sup>523</sup> but his letters to Cavendish at the time imply that there may have been a different story. Then, in Breda, he was quite by chance caught up in the political manoeuvring of 1649-50. It is not easy to explain his later acceptance of a post under Cromwell, but perhaps it seemed expedient to bend before the prevailing political wind, especially for a salary of £600. In the years 1658-60 as the Restoration began to seem a possibility, Pell would have had time to readjust his

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<sup>520</sup> Vaughan 1839.

<sup>521</sup> MS Eng.misc. e. 475, f. 180-181.

<sup>522</sup> Pell to Thurloe 20 April 1654, Vaughan 1839, I, 2.

<sup>523</sup> Aubrey 1992, 230.

position once again and may even have been active, as Titus was, in facilitating the King's return. Such are the realities of civil war and personal survival.

Sheldon conferred on Pell the degree of Doctor of Divinity when he himself became archbishop of Canterbury in 1663, and in the same year Pell was elected a Fellow of the Royal Society. For a while his fortunes looked promising, but the high expectations of him were never fulfilled, and over the next ten years he became increasingly insolvent and dependent on the generosity of his friends. In the late 1660s he lodged for a time with John Collins in London and then at Brereton Hall in Cheshire, the home of William Brereton who as a young man had been his pupil at Breda. The final years of his life were spent in London, at times destitute, with little to show for a lifetime of mathematical labour.

### **Pell's mathematics**

Pell's most significant mathematical work was published in 1668 when the *Teutsche Algebra* of Johann Rahn, his student in Zurich, was translated into English by Thomas Brancker.<sup>524</sup> There can be little doubt that Pell influenced and contributed to the original, and he certainly added new material to the English translation. The title page, however, announced only that it was 'Translated out of the High-Dutch into English by Thomas Brancker MA. Much Altered and Augmented by D.P.' Just as in his student pamphlets forty years before, Pell was reluctant to reveal his name. The original of the title page in Pell's neat hand, exactly as it was later printed, survives among his papers;<sup>525</sup> the initials 'D.P.' have been carefully crossed out, then reinserted, as though even now Pell hesitated as to how much to give away. Yet Pell's contemporaries knew well enough that he was working on the book, indeed Brancker's preface describes just which pages were Pell's (late) contributions. Pell's attitude was not merely one of modesty, for although he was reluctant to claim the book as his own, he had no wish to go completely unnoticed. His somewhat paradoxical attitude is perhaps

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<sup>524</sup> Rahn 1659; Rahn-Pell 1668.

<sup>525</sup> British Library Add MS 4414, f. 2.

most clearly revealed in a paragraph he wrote (but never sent) to Brancker in 1666.<sup>526</sup>

I know not how my mind may alter: but for the present, I think it best not to name mee at all in the title or preface: and yet you may be more ingenious than Rhonius was and not vent all for your owne devices. You may say, that the alterations and additions etc. were made by the advice of one of good reputation in those studies etc.

The story of the publication of the Rahn-Pell *Algebra* has been fully told by others;<sup>527</sup> it depicts Pell as a touchy and difficult personality who had to be approached with endless tact and patience. Pell's colleagues, Brancker, Collins and later Wallis, must have been perplexed at times as to how to avoid naming him without 'venting all for their owne devices', and it is perhaps a measure of the respect in which they held him that they tried so patiently to accommodate his wishes.

The opening of the Rahn-Pell *Algebra* which explains the notation and rules for handling and simplifying equations can probably be attributed to Rahn rather than Pell. Collins, who saw the book through the press, said as much when he wrote to Wallis in 1667:<sup>528</sup>

As concerning the book of Dr. Pell's scholar, I think the Dr. did little concern himself in it till the introduction was past, and to speak plainly, I account that introduction much worse than Principia Matheseos Universalis.

The *Principia matheseos universalis* of the Danish mathematician Erasmus Bartholin was an introductory text on algebra first published in Leiden in 1651 and reprinted in Van Schooten's edition of Descartes ten years later.<sup>529</sup> (A

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<sup>526</sup> Pell to Brancker 11 April 1666, British Library Add MS 4278, f. 80, cited in Scriba 1974, 268.

<sup>527</sup> Scriba 1974.

<sup>528</sup> Collins to Wallis 2 February 1667, Rigaud 1841, II, 472.

<sup>529</sup> Bartholin 1651 and Van Schooten 1661, 1-48. Bartholin had visited England and was personally known to some of the English mathematicians. He sent both Wallis and Oldenburg copies of his treatise on Iceland spar, Bartholin 1669, and Wallis's copy, signed by Bartholin, is now Savile G.25.

‘Cambridge scholar’ discussed Bartholin’s text with Collins in a London bookshop early in 1667, and declared it superior to the opening of the Rahn-Pell *Algebra*,<sup>530</sup> and Newton himself, many years later, also endorsed it as one of the books to be read as a prelude to his own *Principia*.<sup>531</sup>) Collins evidently raised his concerns about the introductory material with Pell while the book was still in the press, but Pell was disinclined to change anything, and Collins, with remarkable loyalty, defended Pell in public, despite his private doubts.<sup>532</sup>

I know none that account the Introduction a bad one, but divers that think it might have been more plain, and ought to have been more large than it is. This is the judgement of divers of the virtuosi and of some teachers of the mathematics here, who all love and honour the Doctor [Pell]; and I hope I shall do no less as long as I live, albeit I am of their mind, nor do I endeavour to make others of the same opinion, but say to them the Doctor did not much concern himself therein, but lets it come out as his scholar left it;

After the introduction, the remaining and greater part of the Rahn-Pell *Algebra* is devoted to ‘The Resolution of divers Arithmetical and Geometrical Problemes’. Now the work is recognisably Pell’s, for almost all of it is set out in the three-column layout which was his invention and hallmark: each line of working is set out in a wide right hand column, preceded by a line number in a narrow centre column and an instruction in the left hand column, much as in a modern computer programme. The unknowns and the conditions of the problem (matching in number if the problem is uniquely determined) are set out in the first few lines of the first and third columns respectively, after which the work proceeds methodically and logically to the solution. Pell devised various useful

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<sup>530</sup> Collins to Pell 9 April 1667, Rigaud 1841, I, 125-126. Whiteside 1968, 279, suggested that the scholar may have been Barrow, but it seems not impossible that it was Newton, see also note 531.

<sup>531</sup> Newton to Bentley c. July 1691, Turnbull 1959-77, III, 155-156. Turnbull, note 5, suggested that the Bartholin text was the *Selecta geometrica* of 1674 but Newton specifically referred to the 1661 Van Schooten edition.

<sup>532</sup> Collins to Brancker June [1667], Rigaud 1841, I, 134-135.

abbreviations for his ‘instruction’ column, for example, the + sign which has since passed into common use was invented by him to avoid using two lines for fractions. The instructions were not entirely consistent: ‘33 + 10p’ meant add 10p to each side of the equation in line 33, and ‘95 + 4’ meant divide through line 95 by 4, but ‘28-80’ could mean subtract line 80 from line 28. However, the meaning was usually clear from the context. Wallis was much impressed by this style, which he called a *method*, and used it himself in the early 1660s. In *A treatise of algebra* he set out a long example taken directly from the Rahn-Pell *Algebra*.<sup>533</sup>

Pell’s method was ideally suited to treating indeterminate problems, with which he was thoroughly familiar from Diophantus. He noted that ‘whensoever the number of required equations is greater than the number of given ones; the question is capable of innumerable answers’,<sup>534</sup> and proceeded as, for example, in Problem XV: ‘To make a rectangled [right-angled] triangle where one leg is equal to the square of the other leg’. Pell used *b*, *c*, *h* to represent the lengths of the legs and hypotenuse of the triangle and began the working as follows:

$h = ?$	1.	$hh = bb + cc$
$b = ?$	2.	$c = bb$
$c = ?$	3.	(*)

The (\*) in line 3 indicates that there is no further condition but since there are three unknowns the solver is later allowed to replace (\*) with an arbitrary condition to aid the solution. The discussion of indeterminate problems is the most interesting feature of the Rahn-Pell *Algebra*; it was this (together with Pell’s layout) that Wallis particularly noted about it, and it led him into a more general discussion of such problems in both arithmetic and geometry (locus problems).<sup>535</sup>

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<sup>533</sup> *Treatise of algebra*, 219-224; Rahn-Pell 1668, 116-124.

<sup>534</sup> Rahn-Pell 1668, 80.

<sup>535</sup> *Treatise of algebra*, Chapters 58, 64-65.

Until now the Rahn-Pell *Algebra* has seemed to contain as much as was ever printed of Pell's mathematics (apart from his refutation of Longomontanus). Inspection of Pell's papers, however, tells a different story. In 1685, the year of Pell's death, a number of problems on which it is now obvious that he had worked either independently or in collaboration with Wallis appeared in *A treatise of algebra*, though none was explicitly ascribed to him. In fact mathematical discussion between Wallis and Pell went on for almost thirty years, and began not long after the time when Wallis had, unbeknown to either, deciphered Pell's letters from Breda. Their earliest communication on mathematics, as opposed to politics, appears to have taken place when Pell was in England between 1652 and 1654, for in 1655 Wallis sent news of his newly published *Arithmetica infinitorum* to Pell in Zurich.<sup>536</sup> It is clear from the manuscripts of both men that they began or resumed a closer relationship after Pell's final return to England in 1658: Wallis's notes on Pascal's *Lettres a Dettonville* which he probably made soon after the book was published in 1659 are written in Pell's three-column layout.<sup>537</sup> Further discussion between Pell and Wallis on a variety of topics continued throughout the 1660s and 1670s resulting in the eventual publication of some of Pell's work in *A treatise of algebra*. This published work will be discussed under the headings of *Silas Titus' problem*, *Dr Davenant's problem*, *Biquadratic equations* and *Representing imaginary numbers*.

### *Silas Titus' problem*

In the middle of his discussion of indeterminate problems Wallis unexpectedly introduced an entirely different kind of question,<sup>538</sup> to find  $a, b, c$  such that:

$$a^2 + bc = 16$$

$$b^2 + ac = 17$$

$$c^2 + ab = 18$$

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<sup>536</sup> British Library Add MS 4418, f. 210.

<sup>537</sup> Pascal 1659; Wallis MS Savile 101, ff. 82-83<sup>v</sup>.

<sup>538</sup> *Treatise of algebra*, Chapters 60-63.



This, he said, had been posed to him in 1662 by Colonel Silas Titus, ‘then of his Majesties Bed Chamber’ (Titus was by then in the service of Charles II). It did not take long to emerge that behind Titus stood Pell: ‘I understood from the Colonel’, wrote Wallis rather vaguely, ‘it was a Question Proposed by Dr Pell’.<sup>539</sup> Given what we now know of Titus and Pell it is no surprise to find that the problem had arisen in Breda in 1649, as Pell later explained in a page which survives amongst his papers:<sup>540</sup>

Mr. William Brereton of Breda anno 1649 brought me an example of this question  $aa + bc = 16$ ,  $bb + ac = 17$ ,  $cc + ab = 22$  . . as triall of logicall skill I transformed it to

$$\begin{aligned}a^2 + bc &= 16 \\b^2 + ac &= 17 \\c^2 + ab &= 18\end{aligned}$$

To which I gave this answer [none is given] but the manner of investigation I did not shew him. Neither do I now at all remember what course I tooke . . but I will heere endeavour to show a way . .

The working quickly peters out but Pell returned to the question on several occasions for it appears scattered through his papers in both the original form ( $cc + ab = 22$ ) and the later version ( $cc + ab = 18$ ).<sup>541</sup>

Pell appears to have discussed the problem again with Titus in 1662 for on 13 December that year he noted that he had left a partial solution for Titus at his house.<sup>542</sup> (Pell was often helpfully meticulous about recording the dates when books or papers changed hands.) Ten days later he left Titus another note about the problem. The calculation in it need not concern us, but the note reveals that a third person was now involved:<sup>543</sup> ‘*He says that  $x$  . . is greater than  $9\frac{21}{62}$  . . and*

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<sup>539</sup> *Treatise of algebra*, 225.

<sup>540</sup> British Library Add MS 4413, f. 52.

<sup>541</sup> For instance in British Library Add MS 4411, ff. 359-367<sup>v</sup>; Add MS 4412 ff. 197<sup>v</sup>-202; Add MS 4413, ff. 38-52; Add MS 4425, ff. 161, 196-206<sup>v</sup>, 377-378.

<sup>542</sup> British Library Add MS 4425, f. 377.

<sup>543</sup> British Library Add MS 4425, f. 378, my italics.

less than  $9\frac{20}{59}$ . I say that . . .  $x$  hath four values.' We do not know who 'he' was but from the solution which emerged four months later and which is to be found among Pell's papers we may surmise that it was Wallis. The solution is written on a single sheet of paper in Pell's handwriting,<sup>544</sup> and in his three-column layout, but a footnote indicates that it was due to Wallis: '*Ex Iohannis Wallisii autographo exscripsi, Aprilis 14. 1663. quod reddidi Cap. Tito. Novemb. 14. 1663.*' ('I wrote this out from a copy in Wallis's hand, April 14 1663, which I delivered to Captain Titus, November 14 1663.') Wallis had succeeded in reducing the problem to the biquadratic equation:

$$e^8 - 80e^6 + 1998e^4 - 14937e^2 + 5000 = 0$$

(where  $e^2 = 2a$ ) and his solution thus far, exactly as it appears in Pell's copy, was later printed in Chapter 60 of *A treatise of algebra*. Wallis introduced it with the words:<sup>545</sup>

The process of [the solution] I drew up in general terms, (after Dr Pell's Method, with which the Colonel was well acquainted,) in this form; (as I find it yet amongst my loose Papers).

Wallis's loose papers on the subject have never been found; only Pell's copy of Wallis's solution now survives.

A few pages later in the same volume of Pell's papers there is further rough working on the problem with the note: '*Ergo conveniunt DIW and MIP in omnibus coefficientibus aequationis*' ('Therefore Dr. John Wallis and Mr. John Pell agree on all the coefficients of the equation'). Wallis, or Pell, or both, went to great lengths to solve the biquadratic equation, and calculated all possible values of  $a$ ,  $b$  and  $c$  to fifteen decimal places. A fair copy in Latin, in Pell's hand, of the entire solution still exists.<sup>546</sup> Exactly the same material, now in English, forms

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<sup>544</sup> British Library Add MS 4425, f. 161.

<sup>545</sup> *Treatise of algebra*, 225.

<sup>546</sup> British Library Add MS 4411, ff. 359-367<sup>v</sup>. Halliwell 1841, xii, attributed the solution without reason to Collins. Halliwell also suggested, but again without supporting evidence, that the problem came originally from Harriot.

Chapters 60, 62 and 63 of *A treatise of algebra* (Chapter 61 is simply a line by line commentary on Chapter 60). This is the only part of Wallis's main text (apart from reprinted letters) for which any manuscript version has been found.

Silas Titus' problem in *A treatise of algebra* runs to over thirty tedious pages and why either Wallis or Pell should have thought it worth so much labour is not clear, for the mathematics was neither new nor of any intrinsic interest. However, the problem refused to fade away. In 1672 Collins sent James Gregory a copy of the 'Breretonian problem',<sup>547</sup> and on 19 October 1677, almost thirty years after Brereton first proposed it, Hooke recorded in his *Diary* that he 'Borrowd of Collins at Rainbow [coffee house], Mr. [Thomas] Bakers solution of problem  $aa+bc=x$ .  $bb+ac=y$ .  $cc+ab=z$ .' Probably the problem came into circulation anew when Wallis, or Pell, decided it deserved a place in *A treatise of algebra*. It appears there under the running title head *Of Dr Pell's Algebra* and, as we have seen, Wallis acknowledged Pell as the originator of the problem, but otherwise gave no indication of the time and effort Pell himself had expended on it.

It is perhaps not out of place here to complete the story of Titus. His mathematical friendship with Pell evidently continued over many years, for we have a record of Pell borrowing sheets of Dechalet's *Cursus* from Titus in 1675 and returning them the following year.<sup>548</sup> Three years after that, in one of the ironic twists of seventeenth-century politics, Titus became the implacable political opponent of Lord Stafford, once the young William Howard for whom Oughtred, with the encouragement of Pell's close friend Charles Cavendish, had written his *Clavis* fifty years before.<sup>549</sup>

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<sup>547</sup> Gregory to Collins 17 January 1672 and 6 August 1672, Rigaud 1841, II, 231, 242.

<sup>548</sup> Dechalet 1674; British Library Add MS 4416, ff. 54-56.

<sup>549</sup> Oughtred 1631. William Howard was executed on the last day of 1680, fifty years, almost to the day, after Oughtred named him in his dedicatory preface of 1 January 1631

### *Dr Davenant's problem*

In Chapters 10 and 11 of *A treatise of algebra*,<sup>550</sup> in a stupendous feat of calculation, Wallis found fractional equivalents for the number we now call  $\pi$ :  $\frac{22}{7}$ ,  $\frac{335}{113}$ ,  $\frac{104348}{33215}$ , . . . ,  $\frac{3076704071730373588}{979345322893700547}$ . Wallis implied that this problem (or something like it) had been put to him about 1663 by Dr Lamplugh, bishop of Exeter on behalf of his father-in-law, Dr Davenant. Edward Davenant was a prebend of Salisbury who, according to John Aubrey, spent a great deal of time on mathematics. Aubrey himself had been taught by him and later wrote that Christopher Wren considered Davenant 'the best Mathematician in the world about 30 or 35+ years agoe'.<sup>551</sup> Davenant certainly did some work on finding fractional equivalents for  $\pi$ , for Collins in 1676 associated his name with the problem,<sup>552</sup> and Wallis suggested that he had found some but not all of the solutions. However, newly discovered evidence suggests that Davenant's role was analogous to that of Titus in the previous problem, and that the question of finding fractional equivalents for  $\pi$  was first raised, and partly solved, by Pell, when he was twenty-five and teaching in Sussex. Pell's work is to be found on a sheet dated 1636 which begins as follows:<sup>553</sup>

Of the proportion of y<sup>e</sup> periphery to y<sup>e</sup> Diameter of a circle

Archimedes (about 210 years before Christ) determined it as 22+ to 7  
Ludolph van Ceulen (Anno Christo 1599) determined it as 314159 26535  
89793 to 1  
Lansberg (Anno Christo 16..) determined it as [29 figures given]  
Henry Briggs (Anno Christo 16..) determined 40 cyphers

So that to seeke a greater proportion is meerely needlesse, it is better to seeke to bring it to some smaller one for common use.

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<sup>550</sup> Wallis's work in these two chapters has been thoroughly explored in Fowler 1990.

<sup>551</sup> Aubrey 1992, 82-84.

<sup>552</sup> Collins to Baker 19 August 1676, Rigaud 1841, II, 8-9. This is the only mention in the Rigaud letters of the fraction problem. Other references by Collins to 'Dr Davenant's problem' are to an entirely unrelated problem, to do with polynomial equations.

<sup>553</sup> British Library Add MS 4416, ff. 31-31'.

1. Some doe by cutting off as many cyphers as they thinke Goode from y<sup>e</sup> end, so Mr Oughtred saise 3[1416 and Mr Gunter 314 [figures lost in binding].

2. Some seeke y<sup>e</sup> ratio in some other Diameter, hoping by y<sup>e</sup> means to find it rationall, so [blank] determined it to be as 355 to 113.

Concerning which we will enquire

1. How farre this proportion of 355 to 113 will hold?  
[Pell decides it will hold as far as 6 ‘cyphers’, or places]

2. How he found this proportion in small numbers?

The fraction 355/113 for  $\pi$  was found by Adriaen Anthonisz Metius in 1584 and was published by his son Adriaen in his *Arithmeticae et geometriae practica* of 1611.<sup>554</sup> Pell appears to have read Metius’s book but to have temporarily forgotten the name of the author. But now compare his second question with Wallis’s words at the beginning of Chapter 10: ‘I find some have been wondring by what means Metius came to light upon those Numbers’. Wallis’s next sentence: ‘I guess that somewhat of this nature did first put Dr Davenant upon this inquiry’ then seems little more than a rather clumsy attempt to shift attention away from Pell. From Pell’s manuscript, it becomes clear that he posed not just the problem but the method of solution, for he continued:

. . . to reduce this fraction  $\frac{3141592}{1000000}$  to an equall one of its least termes, this he might doe by a perpetuall dividing y<sup>e</sup> greater by y<sup>e</sup> lesser, and y<sup>e</sup> divisor by y<sup>e</sup> relique.

Pell recognised that the fraction so obtained was still too large and eventually arrived at his best approximation,  $3\frac{599779526}{423590883}$ . Wallis later used the same method, essentially the Euclidean algorithm, to arrive at his own fractions. He worked in much greater detail and continued further, and he alone may have been responsible for extending Pell’s method, but it remains a possibility that Pell himself did further work in papers now lost. The extensive calculations required are of a kind that both Pell and Wallis handled with ease.

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<sup>554</sup> Metius 1611, 69; Metius 1626, 88-89.

Wallis's solution was printed as an appendix to the second (but not the first) edition of the papers of Jeremiah Horrocks in 1678,<sup>555</sup> and Wallis referred to this published version in *A treatise of algebra*, so the relevant chapters, 10 and 11, must have been added to his text after it was first delivered to Collins in 1677. Their content is not described in the *Proposal* to print *A treatise of algebra* circulated in 1683, suggesting that they were a very late addition, which perhaps explains why they sit somewhat uncomfortably among the surrounding material. The subject of finding fractional equivalents for  $\pi$  was one of the few topics in *A treatise of algebra* to be taken up by later mathematicians: it was noted by Huygens and by Euler, and in the twentieth century has been thoroughly explored by Fowler.<sup>556</sup> No one has recognised until now, however, that Pell suggested both the problem and the method of solution fifty years before they appeared in *A treatise of algebra*.

### *Biquadratic equations*

In 1637 Descartes had given a rule for solving biquadratic equations by writing them as the product of two quadratics<sup>557</sup> but, Wallis later complained, 'How he came by that Rule, he doth no where tell us, nor give us any Demonstration of it.' To complete what Descartes had left unfinished, Wallis proceeded to show how the factorisation should be carried out: first remove the term in  $x^3$  (always possible) and suppose that the biquadratic can be written (in modern notation) as:

$$x^4 + px^2 + qx + r = (x^2 - yx + b)(x^2 + yx + d).$$

Equating coefficients of  $x^4$ ,  $x^2$  and  $x^0$  then leads to  $y$ ,  $b$ ,  $d$  in terms of  $p$ ,  $q$ ,  $r$ . (The equation for  $y$  is the cubic equation to which the Ferrari-Cardano method also gives rise.)

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<sup>555</sup> Wallis 1678.

<sup>556</sup> Huygens 1888-1950, XX, 392-394; Euler 1988-90, I, 326; Fowler 1990.

<sup>557</sup> Descartes 1637, 383-386.

Wallis claimed that he had discovered this method in 1648 and communicated it to his Cambridge contemporary John Smith,<sup>558</sup> but his correspondence with Smith has not survived (Smith died in 1652). In his *Treatise on angular sections*<sup>559</sup> begun in 1648, Wallis dealt with special cases of biquadratic with only fourth powers, squares and constants (and therefore essentially higher quadratics) but it seems very unlikely that at this early date he went any further. With Oughtred his only guide, there is nothing to suggest that he had either the notation or the concepts that would have enabled him to factorise biquadratics. Oughtred's notation did not stretch as far as fourth powers, and Wallis had not yet read Harriot's work on the composition of polynomials.

By the time Wallis wrote *A treatise of algebra* thirty years later, Descartes' solution for biquadratics had been amply expounded elsewhere. It was clearly set out in Gerard Kinckhuysen's *Algebra ofte Stel-Konst* of 1661, but Wallis appears not to have read Kinckhuysen's text, for when he set out his own explanation of Descartes' rule for Collins in 1673 he said that 'all [Descartes'] commentators have been so kind as not to give us any account of the grounds of it.'<sup>560</sup> (Wallis eventually acquired Mercator's handwritten Latin translation of Kinckhuysen's *Algebra* but probably only after Collins gave up hope of publishing it, about 1676.<sup>561</sup>) Wallis, however, had another source for his explanation, for Pell had tackled the same problem long before. A neatly written folded sheet in Pell's handwriting and three-column style, setting out the factorisation of biquadratics for all combinations of sign, is to be found, unexpectedly, among the papers of Charles Cavendish.<sup>562</sup> Cavendish died in 1654 so it would appear that Pell had worked on the problem before that date, and therefore several years before it

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<sup>558</sup> *Treatise of algebra*, 209. The same claim was made by Wallis in letters to Collins 8 April 1673 and 12 April 1673, Rigaud 1841, II, 561, 576.

<sup>559</sup> Wallis 1648.

<sup>560</sup> Wallis to Collins 29 March 1673, Rigaud 1841, II, 559. It was Collins who informed Wallis about one of Kinckhuysen's results on cubic equations, see *Treatise of algebra*, 181.

<sup>561</sup> See Scriba 1964, 55; Whiteside 1968, 290.

<sup>562</sup> British Library Add MS 6083, ff. 100<sup>v</sup>-101.

appeared in Kinckhuysen's *Algebra*. This raises new and as yet unanswered questions as to whether Pell and Kinckhuysen were in contact during Pell's years in the Netherlands. At that time Kinckhuysen (c.1625-1666) was a young student, who had already published a treatise on the quadrant and some of the mathematical papers of his teacher, Pieter Wils.<sup>563</sup> In certain respects Kinckhuysen's later work bears some interesting similarities to Pell's. In addition to the factorisation of biquadratics Kinckhuysen also treated problems where there are fewer equations than unknowns and, like Pell, described how one or more unknowns could be fixed at will to aid the solution.<sup>564</sup> Yet (as Wallis observed) neither biquadratics nor indeterminate equations were commonly treated in mid seventeenth-century texts. It may be no more than coincidence that the two men handled such similar material and in the same way, but the possibility of some interaction cannot be ruled out.

Pell certainly had some involvement in various proposals made in the late 1660s and early 1670s to publish Kinckhuysen's work in England. One of the first suggestions was to publish parts of Kinckhuysen's *Algebra* alongside the Rahn-Pell *Algebra* but, for reasons unknown, this idea never materialised.<sup>565</sup> The story of subsequent attempts to publish Mercator's Latin translation of Kinckhuysen has been told in detail by others,<sup>566</sup> but the existence of a contemporaneous English translation has so far gone unnoticed. The evidence for it comes from a note in Pell's handwriting composed in 1672. This appears to be a draft for inclusion in a letter from Oldenburg to Huygens, and was perhaps written in response to a request from Oldenburg for the latest information on the long-awaited Kinckhuysen translation. Pell's note reads:<sup>567</sup>

Appostils [sic], manu Oldenburgii, upon that of April 9 [1672]

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<sup>563</sup> For biographical information on Kinckhuysen and a full bibliography see Kempenaars 1990.

<sup>564</sup> Scriba 1964, 48.

<sup>565</sup> Scriba 1964, 50; Whiteside 1968, 279.

<sup>566</sup> Scriba 1964; Whiteside 1968.

<sup>567</sup> British Library Add MS 4407, f. 118.



Introductio Kinckhuyseni translated into Latin and enlarged by Mr Newton's notes, to serve as an introduction to his general method of Analyticall quadratures.

As soon as Newton's papers about Analyticall quadrature and 20 Dioptic Lectures come to town, that also of Kinckhuysen will be printed.

Kinckhuysen's last book, of Geometricall problems, was transcribed into Latin by a German gunner blown up in trying experiments of fire-work, but the translation in the hands of M. Bernard is fitted for the press.

And Kinckhuysen's Analyticks [algebra] are translated into English, and put into Dr Pell's method by Brancker the publisher of Rhonius his Algebra.

A comparison with the postscript that Oldenburg sent to Huygens a few days later reveals some telling omissions:<sup>568</sup>

As for Kinckhuysen, his introduction has been translated into Latin and will be enlarged with Mr. Newton's notes, to serve as an introduction to his general method of analytic quadratures. When this arrives in London to be printed the aforesaid introduction of Kinckhuysen will also be printed.

Moreover the last book by the said Kinckhuysen, on geometrical problems, has also been translated into latin; this translation is at the moment in the hands of Mr. Bernard, Professor of Astronomy at Oxford, who is editing it for the press.

The unhappy fate of the German gunner has been omitted but so, significantly, has any mention of Pell, or of Brancker's English translation of Kinckhuysen. 'Pell's method' means Pell's three-column layout, and Brancker may have put Kinckhuysen's work into that format when it was expected that it might accompany the Rahn-Pell *Algebra*. This makes the absence of Kinckhuysen's work from the Rahn-Pell text all the more puzzling: if the translation was ready and the publisher was as keen as Collins reported, then only Pell could have refused its eventual inclusion.

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<sup>568</sup> Oldenburg to Huygens 6 May 1672, Hall and Hall 1965-86, IX, 54-55.

To return to Wallis's exposition of the solution of biquadratic equations, it seems possible that the source of his material was not Kinckhuysen but Pell himself, and that his reference to John Smith, like those to Silas Titus and Edward Davenant, was no more than a feint intended to protect Pell from public scrutiny. That Pell was regarded as something of an authority on biquadratic equations is confirmed elsewhere: in 1675 Collins sent him a letter from Dary with a query on splitting a biquadratic equation into two quadratics. A copy in Pell's handwriting is preserved among his papers and Pell has added, as he so often did, a footnote (in red ink) giving information about when the letter was received, curiously referring to himself in the third person.<sup>569</sup>

Mr Collins

I have been lately Trying to break Biquadratique Aequations into two Quadratique ones and I have effected my purpose in a great many, some by the Aliquote parts and some by the Cubicall Maul, But this soure Crabb, I can not deale with by no method etc.

Your ser<sup>t</sup> Mich: Dary

Tower the 8<sup>th</sup> Febr. 1674/5

The Aequation is this

$$+ yyy + 8yyy - 24 yy + 104y - 676 = 0$$

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*William Lord Brereton gave a copy of this to Doctor Pell on Monday about Noone Febr. 22. 1674|5*

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<sup>569</sup> British Library Add MS 4425, f. 60, printed in Halliwell 1841, 105, but Halliwell cited a manuscript title and pagination which are no longer in use. The item printed by Halliwell immediately afterwards is headed 'Note on the solving of equations by John Pell' but the original in Add MS 4432, f. 26, is not in Pell's handwriting and there is no reason to ascribe it to him.

On this occasion Pell made no note of his reply, but it seems significant that Collins passed the query to him rather than to Wallis or any other mathematician.

Pell repeatedly asserted in conversation that he had a method for finding numerical solutions to polynomial equations by means of tables. He even claimed, according to Collins, that Viète's method was, by comparison, 'work unfit for a Christian, and more proper to one that can undertake to remove the Italian Alps into England'<sup>570</sup> (a statement which perhaps put Collins in mind of the same metaphor in connection with Pell himself). Collins tried valiantly and often to explain Pell's method to other mathematicians (sometimes mentioning tables of sines, at other times logarithms),<sup>571</sup> but just as often he despaired at Pell's refusal to communicate it for himself: 'We have been fed with vain hopes from Dr. Pell about twenty or thirty years' or 'Dr. Pell communicates nothing: he once refused me a proposition, and I am resolved never to move him more.'<sup>572</sup> The only known published example of Pell's numerical method is from a sheet of paper written in 1676, originally enclosed in a letter from Wallis to Collins,<sup>573</sup> and therefore yet another indication of the close relationship between Pell and Wallis during these years. Further examination of Pell's papers will almost certainly reveal more about his method.<sup>574</sup>

### *Imaginary numbers*

Immediately following his discussion of indeterminate problems (arising from his account of the Rahn-Pell *Algebra*) Wallis turned to the question of finding geometrical constructions for imaginary numbers. The possibility of representing

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<sup>570</sup> Collins for Leibniz, undated, Rigaud 1841, I, 248.

<sup>571</sup> For example, Collins to Wallis 2 February 1667; Collins for Leibniz, undated, Rigaud 841, II, 472-473; I, 243-248. The similarity of content between these two letters suggests that they were written at about the same time. See also Collins to Gregory [1670] and 25 March 1671; Collins to Wallis 21 March 1671, Rigaud, II, 198-200, 219-220, 526-527.

<sup>572</sup> Rigaud 1841, II, 195.

<sup>573</sup> Wallis to Collins 16 September 1676, Rigaud 1841, II, 601-603.

<sup>574</sup> One of several questions which deserves closer study is how far Pell's work on equations was constructed on groundwork laid down by Harriot, made available to Pell through Warner.

imaginary numbers in this way had not been explored before, and Wallis devoted four chapters (66 to 69) to it, despite his own assertion that such constructions were ‘beside the present business; which is to consider of pure Algebra from its own principles, abstracted from Geometry.’<sup>575</sup> The earliest hint of Wallis’s thinking on the subject is found in letter to Collins in 1673,<sup>576</sup> but the work was considerably more developed by the time it appeared in *A treatise of algebra*.

Wallis put forward the ingenious suggestion that just as a real number may be thought of as a mean proportional (geometric mean) between two positive numbers (eg  $a^2 = bc$ ) so an imaginary number may be considered a mean proportional between a positive number and a negative (eg  $a^2 = -bc$ ). This idea enabled him to give a geometric interpretation of positive and negative squares in terms of sines and tangents (regarded as lines whose length varied in relation to a given angle). Developing the possibilities of geometrical representation still further, Wallis came up with a range of other constructions, some of which were precursors to the modern Argand diagram. He argued, for instance, that whereas real numbers can be represented on a line, imaginary numbers can only be represented in the surrounding plane, and their distance from the real number line is a measure of their ‘impossibility’ or, as we should now say, their imaginary part. Further constructions showed that if real roots were represented on a circle, imaginary roots could be represented on a related hyperbola. Wallis produced no fewer than ten such methods of representation.

There is nothing that overtly connects any of this work with Pell. However, in a final summary Wallis explained that the value of the constructions is to indicate just how far imaginary, or ‘impossible’, solutions deviate from the real, or ‘possible’:<sup>577</sup>

We find therefore, that in all Equations, whether Lateral or Quadratick, which in the strict sense, and first Prospect, appear Impossible; some mitigation is to

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<sup>575</sup> *Treatise of algebra*, 272.

<sup>576</sup> Wallis to Collins 6 May 1673, Rigaud 1841, II, 578-579.

<sup>577</sup> *Treatise of algebra*, 272.

be allowed to make them Possible; and in such a mitigated interpretation they may yet be useful. . .

In all which [constructions], (and others the like,) the Solution amounts to this; that the case proposed, as to the rigor of it, is impossible: But with such mitigations, it may be thus and thus constructed.

Which while declaring the case in Rigor to be impossible, shew the measure of the impossibility; which if removed, the case will become possible.

Compare these words to a paragraph written by Collins around 1670 or slightly later:<sup>578</sup>

These impossible roots, saith Dr. Pell, ought as well to be given in number as the negative and affirmative roots, their use being to shew how much the data must be mended to make the roots possible, and give points or bounds in delineation, shewing how much a curve must pass beneath or beyond a given right line, by aid whereof the roots are found.

Pell's mysterious method of solving equations numerically, mentioned earlier, seems to have depended on systematically changing the parameters of the equation and noting the point where 'impossible' roots became 'possible' or *vice versa*. Both Pell and Wallis were therefore concerned with the 'measure of the impossibility', the difference between the 'impossible' and 'possible' cases. The similarity of the ideas expressed by Wallis and Pell can only lead to the conclusion that they at least discussed the representation of imaginary numbers, and perhaps jointly explored the various methods of construction that Wallis presented. It seems more than mere coincidence that Wallis inserted this piece of work, which he himself admitted did not really belong in his book, immediately after his section on Pell.

### **Some unanswered questions**

We can now say with considerable certainty that substantial sections of *A treatise of algebra* contain work whose origins can be traced, at least in part, back to Pell.

If the chapters on Harriot are included, then no fewer than forty of Wallis's hundred chapters were to a greater or lesser extent influenced by Pell, an aspect of *A treatise of algebra* which has never before come to light. The present account has done little more than indicate how much research is still needed, for the discovery of Pell's input raises many questions. Was it Wallis or Pell who suggested including so much of this work? Or was it Collins who saw Wallis's treatise as an opportunity for getting some of Pell's mathematics finally into print? Collins hinted to Gregory in 1675 that Pell was 'engaged to publish his papers',<sup>579</sup> and was likely to have known more of the matter than anyone. Why, then, was Pell's name so conspicuously underplayed? Cynical observers might have thought that Wallis hoped to pass the work off as his own: he was censured more than once for too readily publishing other people's work,<sup>580</sup> but his motive in doing so was generally to ensure that others were properly acknowledged, and nothing but generosity can be claimed for his treatment of Harriot, Oughtred, Neile, Brouncker, Newton and many lesser figures. Given what we know of Pell's difficult and 'uncommunicative' nature it seems much more likely that it was Pell himself who insisted on discretion.

The discovery of so much of Pell's mathematics in *A treatise of algebra* inevitably leads to speculation about other ways in which Pell may have influenced Wallis, though the answers may never fully come to light. Two questions of interest are: how far was it Pell who persuaded Wallis to write such a spirited defence of English mathematicians, and to write it in English? Pell, in common with other members of the Royal Society, was keen to promote English over Latin: he ignored Collins' suggestion that he should include a Latin preface to the Rahn-Pell *Algebra* 'to explain the symbols, and to signify that the greatest

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<sup>578</sup> Collins to unknown recipient, Rigaud 1841, II, 481.

<sup>579</sup> Gregory to Collins 23 July 1675, Rigaud 1841, II, 268.

<sup>580</sup> Collins to Wallis 20 September 1677: 'You lye under a censure from diverse for printing discourses that come to you in private Letters without permission or consent as is said of the parties concerned', Turnbull 1959-77, II, 242. See also Aubrey 1898, II, 281-282.

part of the book may be understood by others, ignorant of our tongue'.<sup>581</sup> In a note written after Mercator's *Logarithmotechnia* appeared in 1668, Pell referred judgement of the mathematics to Wallis, but criticised Mercator's choice of language:<sup>582</sup>

In his title page, [Mercator] says his *Logarithmotechnia* had beene communicated in writing in August 1667. He says not to whom. Not to Dr. Wallis, I beleeeve. I desire to know what Hee saith of it. Howsoever (as I wrote before) I look y<sup>t</sup> some transmarine pen should fly at him. Englishmen, perhaps, will let him alone till he print the same crudities in English.

Pell may well have played some part in encouraging Wallis to write not only in English, but about the English. He and Wallis, with their common background in undercover politics, would have had been more aware than most, even at a time of general suspicion, of the dangers of French Catholic dominance. A historical account of algebra was the perfect medium for acclaiming the work of Harriot and other English mathematicians, and if Wallis needed any encouragement Pell would surely have provided it.

Was Pell's contemporary reputation for mathematics justified? From his surviving papers he emerges as a competent but not brilliant mathematician, one who was largely content to work on problems posed to him by others or which arose in the course of his reading. No significant new result can be attributed to him. In his own eyes his most important achievement was his numerical method for solving equations, but he was so secretive about it that to this day no one knows exactly what it entailed, though it does seem to have required extensive calculation of supporting tables. (Pell was a table-maker *par excellence* but the only one he ever published was his table of 10,000 squares.<sup>583</sup>) He also understood analytic methods of solving equations (as learned from Viète, Harriot and Descartes) and his facility in algebra obviously impressed his contemporaries, but

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<sup>581</sup> Collins to Pell 9 April 1667, Rigaud 1841, I, 126.

<sup>582</sup> British Library Add MS 4415, f. 2.

<sup>583</sup> Pell 1672.

he applied his skills only within a relatively limited area, leading Collins to write in the letter already partly quoted:<sup>584</sup>

As to his knowledge, I take him to be a very learned man, more knowing in algebra, in some respects, (which I think I can guess at,) than any other, and they in other respects than he; but as to other parts of the mathematics, I grossly mistake if divers of them do not parasangis bene multis surpass him;

Where Pell was unusual among his contemporaries was in his sense of a deep structural logic in mathematics. This is most clearly evident in his three-column ‘method’ which conveys a feeling for precision and rigour, in contrast to the more descriptive and *ad hoc* styles of argument used by Wallis and others. The concept of mechanical computability was still some centuries ahead, yet Pell’s method is only a step or two away from a computer algorithm. Pell’s vision, however, went beyond the working of individual problems to encompass mathematics itself. In his *Idea* he put forward the claim that it should be possible to derive all mathematical knowledge, past, present and future, by an ordered process of logical reasoning:<sup>585</sup>

. . to lay downe such an exact *Method* or description of the processe of Mans reason in inventions, that *afterward* it should be imputed meerly to my negligence and disobedience to my owne lawes, (and not to their insufficiency) if, from my first grounds, seeds, or principles, I did not, in an orderlie waie, according to that prescribed Method, deduce, not onely all that ever is to bee found in our Antecessor’s writing, and whatsoever they may seeme to have thought on, but also all the Mathematicall inventions, Theoremes, Problemes and Precepts, that it is possible for the working wits of our successors to light upon, and that in one certain, unchanged order, from the first seeds of Mathematics, to their highest and noblest applications, as well as to the meanest and most ordinarie.

There is something in this of Viète’s *Nullum non problema solvere* (to leave no problem unsolved), but we do not know how Pell proposed to develop his

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<sup>584</sup> Collins to Beale 20 August 1672, Rigaud 1841, I, 196-197.

<sup>585</sup> Dury 1650, 43, reprinted in Fauvel and Gray 1987, 312.



scheme, for although, according to Brereton, he wrote ‘a quire of papers’ on the subject,<sup>586</sup> his work has not since come to light. Hobbes in England and Descartes on the continent were similarly concerned with general deductive method, not only in mathematics but in philosophy and science,<sup>587</sup> and Pell also prefigured the later ideas of Leibniz who hoped to create an encyclopaedia of all knowledge and a *scientia generalis* by means of which all truth might be discovered.<sup>588</sup> English mathematicians, however, troubled themselves little if at all with such things, indeed Collins feared that Pell’s notions were no more than ‘improbable presumptions’.<sup>589</sup> By the end of the seventeenth century questions about structure and methodology in mathematics were largely washed aside by a flood of new results and ideas, and by the time mathematicians once again came to think about the logical underpinning of their subject, Pell’s name was almost forgotten.

It was a loss to Pell’s contemporaries (as to us) that he so steadfastly refused to publish, but at the time it seems to have done no damage to his reputation. Paradoxically, and unlike most other mathematicians, he may have achieved his standing by *not* publishing, for he gave no grounds for anyone to question his mathematical ability. Intelligent, widely read, and possessed of a quick sharp wit, Pell easily created an impression of erudition which was never actually subjected to public scrutiny. Only now has it become apparent that some of his work *was* published but behind such a veil of discretion that his name has remained hidden for over three centuries. Even Pell could hardly have asked for more.

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<sup>586</sup> Collins to Wallis 2 February 1667, Rigaud 1841, II, 474.

<sup>587</sup> Jesseph 1999, 4.

<sup>588</sup> Ross 1984, 7, 61-62; Aiton 1985, 91-94

<sup>589</sup> Collins to Beale 20 August 1672, Rigaud 1841, I, 197.



## Chapter 7

### The discovery of wonders: reading between the lines of the *Arithmetica infinitorum*

#### Summary

Wallis's *Arithmetica infinitorum* (1655) was his finest piece of work and one of the key books that determined the course of seventeenth-century English mathematics. This chapter explores the background and content of the book and its reception by English and continental mathematicians. It also describes the mathematics that was developed in or from the *Arithmetica infinitorum* and suggests that the book played a more important role than has hitherto been recognised in the fundamental transition of mathematics from geometry to algebra.

Right at the beginning of his Oxford mathematical career Wallis embarked on the work that was to be published in 1655 as the *Arithmetica infinitorum*.<sup>590</sup> It was his masterpiece and after Oughtred's *Clavis* and Harriot's *Praxis* the third and most far reaching of the books which influenced the course of mid seventeenth-century English mathematics. Wallis himself was well aware of the book's importance and devoted the final quarter (Chapters 73 to 97) of *A treatise of algebra* to describing the contents and implications of the *Arithmetica infinitorum*, as worked out in the text itself and as developed by mathematicians from Brouncker to Newton during the ten years

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<sup>590</sup> Wallis 1655b. The *Arithmetica infinitorum* was twice reprinted in Wallis's collected works, in Wallis 1656 and Wallis 1695. Despite its importance the book has never been translated into English. References to the *Arithmetica infinitorum* in this chapter will be to Propositions rather than to page numbers so that they can be used with any edition. Other reference to Wallis's works will generally be to the relevant pages in Wallis 1695, now the most easily available edition.

following publication. From a much longer perspective this chapter revisits the *Arithmetica infinitorum* and reviews its significance afresh.

The *Arithmetica infinitorum* was not an algebra text in the usual sense, concerned neither with solving polynomial equations nor with applying algebra to geometry. For this reason its role in the evolution of algebra has been largely overlooked,<sup>591</sup> yet it played a vital part in the seventeenth-century transition from geometry to algebra. Most general historians of mathematics have restricted their descriptions of the *Arithmetica infinitorum* to Wallis's (inexplicit) use of negative and fractional exponents and his infinite fraction for  $4/\pi$ , but to Wallis's contemporaries and immediate successors these were far from being the book's most important features.<sup>592</sup> Its real value, as Wallis clearly saw, was in its revolutionary methods: in the *Arithmetica infinitorum* Wallis began to re-write some of the most important ideas in contemporary mathematics in terms of arithmetic and algebra rather than geometry, and in doing so gave new impetus to the shift of mathematical paradigm begun by Descartes. By the end of the seventeenth-century, mathematicians were using algebra not just to write polynomial equations or to describe curves but to explore infinite series and the

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<sup>591</sup> The *Arithmetica infinitorum* is not listed in Rider 1982, and Pycior 1997 gives it no consideration apart from a brief mention of negative and fractional indices (see note 592). Both Rider and Pycior have adopted a somewhat restricted definition of algebra: Rider, pp. 1, 16, recognised the gradual emergence of algebra as 'the fundamental language of mathematics' but restricted her bibliography to books concerned with the theory and practice of equations. The focus of Pycior's text is also, almost exclusively, the solving of equations and the acceptance of negative and imaginary roots. Neither approach does full justice to the remarkable generalisation of all mathematics into algebraic form during the seventeenth century.

<sup>592</sup> Fractional exponents were first denoted, albeit clumsily, by Nicole Oresme c.1350 and negative exponents were used by Nicolas Chuquet in his unpublished *Triparty* of 1484; for details see Cajori 1928-29, I, 91, 102. Both fractional and negative exponents were explored by Michael Stifel (Stifel 1543) but without introducing any notation. Simon Stevin (Stevin 1585) invented a notation for fractional indices. The modern notation, a natural extension of Descartes' index notation for integers, was first used by Newton in 1676 (Newton 1676a,b). Thus the story of negative and fractional exponents neither began nor ended with Wallis. The fraction for  $4/\pi$  was independently discovered by Mengoli in about 1659 and published in Mengoli 1672.

properties of trigonometrical and logarithmic quantities. The word *analysis*, which in 1600 had been used to describe an algebraic approach to geometry, had by 1700 come to include the wealth of new material that could now be handled algebraically.<sup>593</sup> The *Arithmetica infinitorum* was a key text in this profound and far-reaching change.

### Cavalieri and Torricelli

The *Geometria indivisibilia* ('The geometry of indivisibles') of Bonaventura Cavalieri, first published in 1635, followed Oughtred's *Clavis* as the second great formative book in Wallis's mathematical career, its impact on Wallis so great that he gave his own book the title *Arithmetica infinitorum* ('The arithmetic of infinites') as a direct parallel. Initially, however, he had no copy of Cavalieri's work (he reported that he searched the bookshops in vain)<sup>594</sup> and learned of the contents only at second hand through Evangelista Torricelli's *Opera geometrica* of 1644 which he first read in 1651.

Cavalieri, born about 1598 in Milan, was a monk of the Augustinian Jesuati order. As a young man he became a disciple of Galileo, who later wrote of him that 'few, if any, since Archimedes, have delved as far and as deep into the science of geometry': on the strength of this recommendation Cavalieri was appointed as the first Professor of Mathematics at Bologna, a post he held from 1629 until his death in 1647. His *Geometria indivisibilia* was completed in 1627 while he was prior of the Jesuati monastery in Parma. Cavalieri's method of discovering areas was by comparison of one figure with another: two figures were proportional in area if every chord cutting the first was in the same ratio to the equivalent chord cutting the second. In Cavalieri's own words:

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<sup>593</sup> Newton in 1669 wrote: 'And whatever common analysis [algebra] performs by equations made up of a finite number of terms . . . this method may always perform by infinite equations: in consequence I have never hesitated to bestow on it also the name of analysis', Whiteside 1967-81, II, 241.

<sup>594</sup> *Arithmetica infinitorum*, 'Dedicatio'.

*Si duae figurae planae, vel solidae, in eadem altitudine fuerint constitutae, ductis autem in planis rectis lineis, et in figuris solidis ductis planis utcumque inter se parallelis, . .*

Given two plane figures (or solids) of the same height, and a series of parallel straight lines (or parallel planes), then if all the straight lines intercept the plane figures (or the planes intercept the solids) in the same proportion, the two figures will themselves be in that same proportion.<sup>595</sup>

The parallel chords (or planes) of which the figures were supposedly constituted were the ‘indivisibles’ of the title.<sup>596</sup> Such ideas were an essential part of the later development of the calculus, but even at the time it was recognised that Cavalieri had opened up a rich new seam of mathematics. In 1655 William Oughtred wrote:<sup>597</sup>

. . full twenty years ago, the learned patron of sciences, Sir Charles Cavendish, shewed me a written paper sent out of France, in which were some very few excellent new theorems, wrought by the way, as I suppose, of Cavalieri, which I wrought over again more agreeably to my way . . I mention it for this, because I saw therein a light breaking out for the discovery of wonders to be revealed to mankind, in this last age of the world:<sup>598</sup>

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<sup>595</sup> Cavalieri 1635, 115, translation JS. For a translation of an earlier section of the text see Evans G.W. 1917.

<sup>596</sup> Giusti 1980; Andersen 1985.

<sup>597</sup> Oughtred to Wallis 17 August 1655, Rigaud 1841, I, 87-88. Among Cavendish’s papers in the British Library is a handwritten treatise *Elements des indivisibles*, Harley MS 6083, ff. 279-302. The text, in French, includes definitions, propositions and diagrams which are clearly based on Cavalieri’s. It is tempting to conclude that this was the same ‘written paper’ that Cavendish showed to Oughtred.

<sup>598</sup> Belief in the imminence of Doomsday was commonplace throughout Oughtred’s lifetime but millenarianism was particularly strong during the Interregnum, 1649-60. The year 1656 was thought by some to be an especially likely date as it represented the number of years supposed to have elapsed between the Creation and the Flood, see Thomas 1971, 140-144.

Oughtred, like Wallis later, also complained that he was unable to procure Cavalieri's book:<sup>599</sup>

. . a geometrical-analytical art or practice found out by one Cavalieri, an Italian, of which about three years since I received information by a letter from Paris, wherein was praelibated only a small taste therof, yet so that I divine great enlargement of the bounds of the mathematical empire will ensue. I was then very desirous to see the author's own book while my spirits were more free and lightsome, but I could not get it in France.

Cavalieri's work was taken up and extended by Torricelli, who supposed that these were the secret methods by which the Greeks had discovered their results and that by reintroducing them, not only would ancient methods be made clear, but new results might be discovered (exactly the hopes that Viète half a century earlier had held of his 'analytic art'). It was Wallis, perhaps introduced to Cavalieri's work by Oughtred, who was to take Cavalieri's ideas furthest, though still with the essential aim of exploring and developing ancient mathematics. In doing so he was to fulfill Oughtred's hope of enlarging the mathematical empire in hitherto unimaginable ways.

### **Squaring the circle**

Wallis's crucial insight was to see how Cavalieri's work could be treated arithmetically. For example, given a triangle and a circumscribed parallelogram, the application of Cavalieri's ideas would yield, in modern ratio notation:

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<sup>599</sup> Oughtred to Keylway 26 October 1645, Rigaud 1841, I, 65-66. This letter implies that Oughtred first saw Cavalieri's work in 1642, rather later than the 'full twenty years ago' of 1655.

$$\begin{aligned}
& \text{area of triangle : area of parallelogram} \\
&= \text{sum of chords in triangle : sum of chords in parallelogram} \\
&= \text{sum of chords in triangle : sum of longest chord taken the same number of} \\
&\hspace{20em} \text{times} \\
&= \text{sum of an arithmetic progression : sum of largest term taken the same} \\
&\hspace{15em} \text{number of times} \\
&= \frac{1}{2} : 1
\end{aligned}$$

(1)

The shift in this argument from a geometric diagram to a calculation in pure arithmetic was at the heart of Wallis's work. He saw that by summing not just arithmetic progressions, but sequences of squares, cubes and higher powers, he could find areas, or quadratures, of curved shapes. And ultimately he hoped to find the quadrature of the circle, a problem whose exact solution had eluded mathematicians since classical times.

Wallis began simply: in Proposition 1 of the *Arithmetica infinitorum*, he tested out a few numbers:

$$\frac{0+1}{1+1} = \frac{1}{2}, \quad \frac{0+1+2}{2+2+2} = \frac{1}{2}, \quad \dots, \quad \frac{0+1+2+3+4+5+6}{6+6+6+6+6+6+6} = \frac{1}{2}$$

In Proposition 2 he gave the first algebraic generalisation of such a result ever to have appeared in print: the formula  $\frac{l+1}{2}l$  for the sum of integers 0 to  $l$ .<sup>600</sup> In

Proposition 3 he applied this result to the area of a triangle and showed, by an argument along the lines of (1), that it was half that of the surrounding parallelogram, confirmation for Wallis that his method worked. On such simple foundations was his edifice built. Even from this first easy formula there were several corollaries: on the ratio of a paraboloid to the circumscribing cylinder and on the area of the spiral whose equation in modern notation is  $r^2 = a\theta$ . In

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<sup>600</sup> The first known appearance of this formula is in the unpublished manuscripts of Thomas Harriot, see, for example, British Library Add MS 6782, f. 108.



Propositions 19 and 20 Wallis derived a formula for the sum of squares from  $0^2$  to  $l^2$ :  $\frac{l+1}{3}l^2 + \frac{l+1}{6l}l^2$ . From this followed the volume of a cone, the area of a parabola, and the first steps towards something previously considered impossible, the rectification of a parabola. Already, with only two formulae in place, Wallis was making clear the scope and variety of the applications of his method, and continued to do so as he moved on to the sum of cubes:  $\frac{l+1}{4}l^3 + \frac{l+1}{4l}l^3$ , and of higher powers.

At Proposition 53 Wallis extended his results by interpolation to fractional powers; at Propositions 78 and 81 to multiplication and division of powers; and at Proposition 87 to negative powers (though without using index notation for either fractions or negatives).<sup>601</sup> At every stage he gave geometrical corollaries to his propositions until he arrived (as Torricelli had before him) at the existence of plane figures or solids which were infinite in extent but finite in area or volume.<sup>602</sup>

All this, however, was hardly more than a lengthy introduction. At Proposition 108 Wallis embarked on a journey which was to take him far beyond his predecessors, to his true objective, the quadrature of the circle. If a quadrant of radius 1 is divided into strips perpendicular to one of its straight edges,<sup>603</sup> the length of a strip at distance  $r$  from the centre of the circle is  $(1 - r^2)^{1/2}$ . To find the area of the quadrant Wallis needed the sum of such strips, in modern notation  $\sum_{r=0}^1 (1 - r^2)^{1/2}$ , letting  $r$  increase in sufficiently small steps, but he had no way of expanding  $(1 - r^2)^{1/2}$  as a sum of powers. Instead, he tackled the problem by a

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<sup>601</sup> Detailed accounts of this part of the *Arithmetica infinitorum* have been given in Nunn 1910-11; Scott 1938, 26-64.

<sup>602</sup> Torricelli had published his result for the solid of revolution of the hyperbola  $xy = 1$  in Torricelli 1644. Further results obtained by him from 1644 to 1647 were never published and would have remained unknown to Wallis.

<sup>603</sup> Wallis actually used a quadrant of radius  $R$  but then found the ratio of its area to the area of the circumscribing square,  $R^2$ , so that  $R$  disappeared from his solution. To simplify the subsequent explanation I have taken  $R = 1$ .

method that must have become second nature to him in the course of his code-breaking, namely, to pin down as many certainties as possible, then to fill in the gaps by interpolation. Wallis could evaluate  $\sum_{r=0}^1 (1 - r^{1/p})^q$  provided  $p$  and  $q$  were integers, and he set out reciprocals of these values in the table shown below.<sup>604</sup> The row  $p = 0$  was unexplained: Wallis made no attempt to define a ‘zero<sup>th</sup>’ root but introduced the row to maintain the symmetry of the table.<sup>605</sup>

Table I. Values of reciprocals of  $\sum_r (1 - r^{1/p})^q$

(*Arithmetica infinitorum*, Proposition 132)

	q=0	q=1	q=2	q=3	Q=4	q=5	q=6
p=0	1	1	1	1	1	1	1
p=1	1	2	3	4	5	6	7
p=2	1	3	6	10	15	21	28
p=3	1	4	10	20	35	56	84
p=4	1	5	15	35	70	126	210
p=5	1	6	21	56	126	252	462
p=6	1	7	28	84	210	462	924

Wallis immediately recognised the sequences of figurate numbers (triangular numbers: 1, 3, 6, 10, . . . , pyramidal numbers: 1, 4, 10, 20, . . . , etc) in the table and began to write his row and column headings to reflect the nature of these entries. He knew that for the circle he needed  $p = 1/2$ ,  $q = 1/2$ , in other words, he needed to interpolate between  $p = 0$  and  $p = 1$ , and between  $q = 0$  and  $q = 1$ . Wallis therefore

<sup>604</sup> Row and column headings are given here in modern notation where Wallis wrote ‘Aequalia’, ‘Residua’, ‘Quadrata’, ‘Cubi’ etc. The original is reproduced in Struik 1986, 250.

<sup>605</sup> The diagonal symmetry of the table is one of its remarkable features, but a general proof requires knowledge of the properties of binomial coefficients, precisely what Wallis did not yet have.

expanded his table and introduced the symbol  $\square$  to represent the ratio of a square to its inscribed circle,<sup>606</sup> in modern notation  $4/\pi$ .

Table II. Wallis's table expanded  
(*Arithmetica infinitorum* Proposition 169)

		Mon.		Lat.		Tri.		Pyr.
Monadici		1		1		1		1
			$\square$					
Laterales		1		2		3		4
Triangulares		1		3		6		10
Pyramidales		1		4		10		20

From now on, for the remaining twenty-five propositions, Wallis dropped any further reference to geometry and devoted his entire attention to filling the gaps in the table and finding a value for  $\square$  by numerical interpolation alone.<sup>607</sup>

First Wallis derived formulae for the triangular numbers:  $\frac{l^2 + l}{2}$ , pyramidal numbers:  $\frac{l^2 + 3l + 2}{6}$ , and so on, but then took the bold step of using the same formulae to find intermediate, non-integer, values. This was a remarkable advance: the figurate numbers had long been known in European arithmetic but always, by definition, as integers.<sup>608</sup> Wallis himself had derived his formula for triangular numbers by considering an array of points on a triangular lattice, in keeping with traditional understanding, but such physical concepts became

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<sup>606</sup> The symbol  $\square$  was perhaps taken from the *Cursus* of Cyriaque de Mangin 1634-42, where it was used to denote a square number. *A treatise of algebra*, 128, cites the *Cursus* as a text which introduced new notation, but refers to the author as Hérigone, see note 296.

<sup>607</sup> See also Whiteside 1961a, 236-241.

<sup>608</sup> Edwards 1987, 1-19.

meaningless for intermediate values. Here, as so often in what was to follow, Wallis simply assumed continuity between the entries in his tables.

Table III. Completion of rows and columns containing figurate numbers  
(*Arithmetica infinitorum*, Proposition 184)<sup>609</sup>

		Mon.		Lat.		Tri.		Pyr.
	$\infty$	1		$\frac{1}{2}$		$\frac{3}{8}$		$\frac{15}{48}$
<b>Monadici</b>	1	1	1	1	1	1	1	1
		1	$\square$	$\frac{3}{2}$		$\frac{15}{8}$		$\frac{105}{48}$
<b>Laterales</b>	$\frac{1}{2}$	1	$\frac{3}{2}$	2	$\frac{5}{2}$	3	$\frac{7}{2}$	4
		1		$\frac{5}{2}$		$\frac{35}{48}$		$\frac{315}{48}$
<b>Triangulares</b>	$\frac{3}{8}$	1	$\frac{15}{8}$	3	$\frac{35}{48}$	6	$\frac{63}{8}$	10
		1		$\frac{7}{2}$		$\frac{63}{8}$		$\frac{693}{48}$
<b>Pyramidales</b>	$\frac{15}{48}$	1	$\frac{105}{48}$	4	$\frac{315}{48}$	10	$\frac{693}{48}$	20

The problem of filling in the remaining spaces can be tackled in more than one way, as we shall see later. Wallis's method was to note that each entry can be obtained by multiplication from the preceding terms. For example in the *laterales* row the (odd-numbered) terms  $\frac{1}{2}$ ,  $\frac{3}{2}$ ,  $\frac{5}{2}$ ,  $\frac{7}{2}$ , . . . are obtained by cumulative multiplication of the first term,  $\frac{1}{2}$ , by  $\frac{3}{1}$ ,  $\frac{5}{3}$ ,  $\frac{7}{5}$ , . . . while the (even-numbered) terms 1, 2, 3, 4, . . . are similarly obtained by cumulative multiplication of 1 by  $\frac{3}{1}$ ,  $\frac{5}{2}$ ,  $\frac{7}{3}$ , . . . (or  $\frac{1}{2}$ ,  $\frac{3}{4}$ ,  $\frac{5}{6}$ , . . .). Wallis's choice of multiplication as his interpolative tool was the determining factor in all that followed. In the end it was to limit his room for manoeuvre, but here initially he used it with great success. For example, in the third row (between *monadici* and *laterales*) the even-numbered terms are  $1 \times \frac{3}{2} \times \frac{5}{4} \times \frac{7}{6} \times \dots$  and he could fill in the odd-numbered terms by analogy as

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<sup>609</sup> For a reproduction of the original see Struik 1986, 252. Note the use of  $\infty$  to represent an infinite quantity. Wallis had introduced this symbol on the opening page of his *De sectionibus conicis*, a book closely connected to the *Arithmetica infinitorum* as will be discussed later.

$\frac{1}{2}\square \times \frac{2}{1} \times \frac{4}{3} \times \frac{6}{5} \times \dots$ <sup>610</sup> By now he had dropped all row or column headings and was working only with the numerical entries.

Table IV. All rows and columns completed  
(*Arithmetica infinitorum*, Proposition 189)

$\infty$	1	$\frac{1}{2}\square$	$\frac{1}{2}$	$\frac{1}{3}\square$	$\frac{3}{8}$	$\frac{4}{15}\square$	$\frac{15}{48}$
1	1	1	1	1	1	1	1
$\frac{1}{2}\square$	1	$\square$	$\frac{3}{2}$	$\frac{4}{3}\square$	$\frac{15}{8}$	$\frac{8}{5}\square$	$\frac{105}{48}$
$\frac{1}{2}$	1	$\frac{3}{2}$	2	$\frac{5}{2}$	3	$\frac{7}{2}$	4
$\frac{1}{3}\square$	1	$\frac{4}{3}\square$	$\frac{5}{2}$	$\frac{8}{3}\square$	$\frac{35}{48}$	$\frac{64}{15}\square$	$\frac{315}{48}$
$\frac{3}{8}$	1	$\frac{15}{8}$	3	$\frac{35}{48}$	6	$\frac{63}{8}$	10
$\frac{4}{15}\square$	1	$\frac{8}{5}\square$	$\frac{7}{2}$	$\frac{64}{15}\square$	$\frac{63}{8}$	$\frac{128}{15}\square$	$\frac{693}{48}$
$\frac{15}{48}$	1	$\frac{105}{48}$	4	$\frac{315}{48}$	10	$\frac{693}{48}$	20

Finally, studying the third row, and taking odd- and even-numbered terms separately, Wallis saw, in a brilliant flash of insight, how the two sequences:

$$\begin{aligned} \frac{1}{2}\square \times \frac{2 \times 4 \times 6 \times \dots}{1 \times 3 \times 5 \times \dots} \\ 1 \times \frac{3 \times 5 \times 7 \times \dots}{2 \times 4 \times 6 \times \dots} \end{aligned} \quad (2)$$

must eventually approach each other. By comparing partial products from each sequence he was able to trap  $\square$  between closer and closer limits, for example:

$$\begin{aligned} \square &< \frac{3 \times 3 \times 5 \times 5 \times 7 \times 7 \times 9 \times 9 \times 11 \times 11 \times 13 \times 13}{2 \times 4 \times 4 \times 6 \times 6 \times 8 \times 8 \times 10 \times 10 \times 12 \times 12 \times 14} \times \sqrt{1 \frac{1}{13}} \\ \square &> \frac{3 \times 3 \times 5 \times 5 \times 7 \times 7 \times 9 \times 9 \times 11 \times 11 \times 13 \times 13}{2 \times 4 \times 4 \times 6 \times 6 \times 8 \times 8 \times 10 \times 10 \times 12 \times 12 \times 14} \times \sqrt{1 \frac{1}{14}} \end{aligned}$$

Wallis then showed, in the first algebraic proof of its kind, that by taking large enough values of  $z$  the difference between the fractions  $\sqrt{1 \frac{1}{z-1}}$  and  $\sqrt{1 \frac{1}{z}}$  can be made less than any pre-assigned quantity and therefore that the two sequences can

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<sup>610</sup> A particular leap of faith is required in the first row where the first multiplication is  $\infty \times \frac{0}{1} = \frac{1}{2}\square$ .

Wallis easily persuaded himself that such a step was permissible.

ultimately be regarded as equal. This idea foreshadowed the modern definition of a limit,<sup>611</sup> but Wallis's use of it was based on his reading of Euclid, as he later explained:<sup>612</sup>

And when . . . [Euclid] had occasion to compare Quantities, wherein it was not easy by direct Demonstration, to prove their Equality; he takes this for a Foundation of his Process in such Cases: that *those Magnitudes* (or Quantities,) *whose Difference may be proved to be Less than any Assignable are equal*. For if unequal, their Difference, how small soever, may be so Multiplied, as to become Greater than either of them: And if not so, then it is nothing.

Hence Wallis could set the two infinite sequences (2) equal to each other and write:<sup>613</sup>

$$\square = \frac{3 \times 3 \times 5 \times 5 \times 7 \times 7 \times etc}{2 \times 4 \times 4 \times 6 \times 6 \times 8 \times etc}$$

$$\text{or } \frac{9 \times 25 \times 49 \times 81 \times etc}{8 \times 24 \times 48 \times 80 \times etc}$$

It is still possible, across more than three centuries, to sense Wallis's mounting excitement as he reached this, the climax of his work, and his pride was understandable, for his result and process he had used were unprecedented. Only five years after beginning any serious study of mathematics, Wallis had more than justified his Oxford appointment, and had established himself as one of the great mathematicians of his day.

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<sup>611</sup> Mahoney 1973, 226 n. 23, suggested that Newton was the first to present this idea as a formal lemma, but Wallis in 1655 was explicit in his statement of it.

<sup>612</sup> *Treatise of algebra*, 282, Wallis's italics. Wallis referred to Euclid 'Book X onwards'; the key proposition is X.1 which is the foundation of several subsequent propositions.

<sup>613</sup> Such a fraction, with infinitely large numerator and denominator, is strictly meaningless but Wallis had clearly described the limiting process by which he arrived at it. In this sense it was much more soundly based than his earlier unrigorous use of infinite quantities.

## Praise and criticism

Oughtred, over eighty years old, greeted the *Arithmetica infinitorum* with delight. In it he saw the first realisation of the hopes he had held out twenty years earlier, and wrote the glowing letter already partly quoted above.<sup>614</sup>

I have with unspeakable delight, so far as my necessary businesses, the infirmness of my healthe, and the greatness of my age (approaching now to an end) would permit, perused you most learned papers, of several choice arguments, which you sent me: wherein I do first with thankfulness acknowledge to God, the Father of lights, the great light he hath given you; and next I gratulate you, even with admiration, the clearness and perspicacity of your understanding and genius, who have not only gone, but also opened a way into these profoundest mysteries of art, unknown and not thought out by the ancients. . . I saw [in Cavalieri's theorems] a light breaking out for the discovery of wonders to be revealed to mankind, in this last age of the world: which light I did salute afar off, and now at a nearer distance embrace in your prosperous beginnings.

Not everyone, however, was so enthusiastic: from John Pell in Zurich and Christian Huygens in the Netherlands came more cautious responses, and from Thomas Hobbes in England and Pierre de Fermat in France outright criticism which forced Wallis to justify the very foundations of his work.

Pell's reaction to the *Arithmetica infinitorum* has never previously come to light but is preserved on a scrap of paper (10 cm x 12 cm) among the 33 volumes of Pell's surviving letters and mathematical notes in the British Library.<sup>615</sup> Pell was in the habit of using the space at the end of letters he received to make a copy of his reply; in this case the original letter is lost but Pell made a copy of the relevant paragraph and his response, and it is of considerable interest as the earliest evidence of mathematical communication between Pell and Wallis. Pell received the *Arithmetica infinitorum* (or perhaps only its opening dedication to Oughtred?) when it was first published in 1655. It was brought to him by an

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<sup>614</sup> Oughtred to Wallis 17 August 1655, Rigaud 1841, I, 87-88.

<sup>615</sup> British Library Add MS 4418, f. 210.

unidentified intermediary who also conveyed an apparently derogatory remark from Wallis about Pell's mathematical abilities. The message, as copied by Pell, read as follows:

Apr 27. 1655

In y<sup>e</sup> meantime accept of Dr Wallis Quadratum Circuli here adjoined, which I intreate you to handle soundly. For hee makes himselfe beleieve you will doe no great matters in Mathematicall studys.

Pell's response, through the same intermediary, was typical of his somewhat clipped and acerbic style:

May 26. 1655

Sir. I thanke you for yours of April 27 with that printed paper inscribed to Mr Oughtred. If his great age have not made him unwilling to looke upon things of that nature, perhaps he will make some reply. When it comes to your hand, I pray you to send it to mee. As also if the Author expresse himself more fully heere-after. Artists will not trouble themselves to make an enquiry concerning the truth of his new Theorems, till they be sure of the sense of it. They may soone find out the mysterie of continuing his numbers as farre as they desire and so may perceive that his Graver hath set 360 for 630. But out of that paper and those schemes, no man will be able to find what he means by aequabilis curva. He makes mention of a Probleme proposed by him, to many mathematicians, some years ago. Perhaps y<sup>t</sup> problem joined with this printed paper, might help toward the finding of his meaning. I never saw that Problem, nor heard of it till now. But I should be glad to see it, especially if it have an intelligible Definition of aequabilis curva, in such a sense as he would have his Readers understand in his new Theorems.

The passage Pell referred to was from the *Dedicatio* in which Wallis described how he sought to interpolate intermediate values in the sequence 1, 6, 30, 140, 630, . . . (this was where Pell noted the misprint). To convey the underlying need for continuity Wallis described such interpolated values as points on a *curva aequabilis*, a uniform curve or, as we should now say, a smooth or



continuous curve, passing through ordinates 1, 6, 30, 140, . . . at equal intervals from the origin. It is not clear why Pell had such difficulty with this. Wallis stated in the same *Dedicatio* that in 1652 he had proposed the problem to his Oxford colleagues Seth Ward, Lawrence Rooke (who became Gresham Professor of Astronomy that same year), Richard Rawlinson of The Queen's College, Robert Wood of Lincoln College and Christopher Wren of All Souls. Pell returned from Breda only in 1652 and was not part of this illustrious group.

Huygens received the book a year later than Pell, perhaps as part of Wallis's newly published collected works. In his letter of thanks<sup>616</sup> he expressed some uncertainty about the validity of Wallis's result  $\square = \frac{3.3.5.5.7.7}{2.4.4.6.6.8}$  &c., and also about the method of induction which, he said, was neither clear nor certain enough to resolve his doubt. An additional query about Wallis's limiting process in Proposition 191 suggested that he had not actually read it very carefully. In his reply<sup>617</sup> Wallis showed that his expression for  $\square$  agreed with previously calculated values to nine decimal places. He also claimed that 'induction' was not a new method but had been used by Briggs, Viète and even Euclid. 'Induction' in Wallis's sense was not anything so rigorous as the modern principle of mathematical induction, but an argument from precedent, the principle that a pattern or procedure once established could be continued indefinitely.

The discussion with Huygens went no further,<sup>618</sup> but in Oxford the philosopher Thomas Hobbes launched a far more virulent attack. Hobbes' criticisms have to be seen in the context of what was to become a wide-ranging (and long-running) dispute between him and Wallis arising from their very different theological and social conceptions, in which the nature and status of mathematics was just one of the issues at stake.<sup>619</sup> After the Restoration the

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<sup>616</sup> Huygens to Wallis 21 July 1656, no. 316 in Huygens 1888-1950, I, 458-460.

<sup>617</sup> Wallis to Huygens 22 August 1656, no. 325, *ibid.* 476-480.

<sup>618</sup> The final letter in the sequence was Huygen's brief acknowledgment to Wallis, no. 337, *ibid.* 494-495.

<sup>619</sup> Cajori 1929; Pycior 1987; Grant H. 1990, 1996; Probst 1993; Jesseph 1993, 1999.

tensions with Hobbes extended beyond Wallis to other members of the Royal Society,<sup>620</sup> but in 1656 the *Arithmetica infinitorum* was caught in the crossfire of the first round: Hobbes' attack came in his *Six lessons to the professors of the mathematicks* in 1656; Wallis's reply was *Due correction for Mr Hobbes, or school discipline for not saying his lessons right*. The titles are a fair indication of the quarrelsome and personal nature of the exchange. In *Six lessons*, Hobbes upheld geometry as the foundation of mathematical reasoning, and, in striking contrast to Oughtred's view that symbolism 'plainly presenteth to the eye the whole course and process of every operation',<sup>621</sup> Hobbes complained that 'Symboles serve only to make men go faster about, as greater Winde to a Winde-mill.'<sup>622</sup> In lesson five, he explained at more length:<sup>623</sup>

I shall also add that symboles though they shorten the writing, yet they do not make the reader understand it sooner than if it were written in words. For the conception of the lines and figures (without which a man learneth nothing) must proceed from words either spoken or thought upon. So that there is a double labour of the mind, one to reduce your symbols to words, which are also symbols, another to attend to the ideas which they signifie.

Hobbes lacked Wallis's technical competence in mathematics. When, for example, he gave the sequence 0, 1, 3, 5, 7, . . . as a counter example to Wallis's rule for the sum of an arithmetic progression, it was easy for Wallis to point out that this was no arithmetic progression at all. Hobbes struck more telling blows, however, against two fundamental concepts of the *Arithmetica infinitorum*: induction and indivisibles. Hobbes criticised those 'Egregious logicians and geometricians, that think an Induction without a Numeration of all the particulars sufficient to infer a Conclusion universall, and fit to be received for a Geometricall Demonstration.'<sup>624</sup> Wallis replied that induction was a perfectly

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<sup>620</sup> Shapin and Schaffer 1985.

<sup>621</sup> Oughtred 1647, 'Preface'.

<sup>622</sup> Hobbes 1656, Introduction.

<sup>623</sup> Hobbes 1656, 54.

<sup>624</sup> Hobbes 1656, 46.

valid method ‘if after enumeration of some particulars comes the general clause “and the like in other cases”’,<sup>625</sup> otherwise, argued Wallis, none of Euclid’s propositions could be considered proved, for it was impossible to demonstrate every separate case.

With regard to indivisibles, Hobbes insisted that they must be either ‘somewhat or nothing’ and particularly objected to the way Wallis treated an arbitrarily narrow strip *quasi linea* (as if it were a line):<sup>626</sup>

‘The triangle consists as it were’ (‘as it were’ is no phrase of a geometrician) ‘of an infinite number of straight lines.’ Does it so? Then by your own doctrine, which is, that ‘lines have no breadth’, the altitude of your triangle consisteth of an infinite number of ‘no altitudes’, that is of an infinite number of nothings, and consequently the area of your triangle has no quantity. If you say that by the parallels you mean infinitely little parallelograms, you are never the better; for if infinitely little, either they are nothing, or if somewhat, yet seeing that no two sides of a triangle are parallel, those parallels cannot be parallelograms.

These were criticisms neither Wallis nor anyone else was to answer satisfactorily for a long time. Wallis’s response, ‘I do not mean precisely a line but a parallelogram whose breadth is very small, viz an aliquot part [divisor] of the whole figures altitude’,<sup>627</sup> contradicted his own assertions elsewhere that they could be infinite in number. The arguments went on through two further pamphlets, Hobbes’ *ΣΤΙΓΜΑΙ* and Wallis’s *The undoing of Mr Hobs’s points*, but to no further avail.<sup>628</sup> Wallis’s tone, as in all his writing to Hobbes, was scathing: ‘And tis to be hoped you may, in time, learn the language;’ he remarked, ‘for you be come to great A already.’<sup>629</sup>

An attack from a mathematician who had progressed far beyond the letter A came the following year. Fermat remarked in April 1657 that he had found the

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<sup>625</sup> Wallis 1656a, 42.

<sup>626</sup> Hobbes, 1656, 46.

<sup>627</sup> Wallis 1656b, 43.

<sup>628</sup> Hobbes 1657; Wallis 1657b.

<sup>629</sup> Wallis 1656b, 49.

quadrature of the hyperbola himself many years since, though Wallis, he added patronisingly, was no doubt unaware of it.<sup>630</sup> Fermat was correct on both points: he had corresponded with Torricelli on the quadrature of the hyperbola (and other curves) in 1646 but Wallis could not possibly have known it as Fermat had written nothing publicly on quadrature since 1636.<sup>631</sup> He added that he was not fully persuaded of Wallis's result on the quadrature of the circle. More detailed criticisms soon followed.<sup>632</sup> Fermat made it clear that he, like Hobbes, much preferred the geometrical methods of the ancients, which he himself had followed, to Wallis's new ways:<sup>633</sup>

*Ce n'est pas que je ne l'approuve, mais toutes ses propositions pouvant estre démontrées via ordinaria, legitima et Archimedeana en beaucoup moins de parolles, . .*

It is not that I do not approve it, but all his propositions could be proved in the usual, regular Archimedean way in many fewer words than his book contains. I do not know why he has preferred this method with Algebraic notation to the older way which is both more convincing and more elegant, as I hope to make him see at my first leisure.

Fermat appended his *Remarques sur l'Arithmétique des Infinis du S.J. Wallis* containing four specific charges:<sup>634</sup>

1. That Wallis hoped to find the ratio of a sphere to the circumscribing cylinder. But, argued Fermat, this was only possible if one knew first the ratio of the circle to the square, precisely what Wallis was trying to find.
2. That it was impossible to interpolate new numbers into the series 1, 6, 30, 140, . . . for if the series was regarded as being generated by cumulative

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<sup>630</sup> Fermat to Digby 20 April 1657, in Wallis 1658, no. 4.

<sup>631</sup> Mahoney 1973, 217 n. 5.

<sup>632</sup> Fermat to Digby 15 August 1657, in Wallis 1658, no. 12.

<sup>633</sup> For Fermat's application (and modification) of Archimedean methods to the quadrature of the spiral  $r^2 = a\theta$  and other curves in 1636, see Mahoney 1973, 218-228 and 233-239.

<sup>634</sup> Fermat to Wallis August 1657, in Wallis 1658, no. 13.

multiplication of 1 by  $\frac{6}{1}, \frac{10}{2}, \frac{14}{3}, \frac{18}{4}, \dots$  which Wallis wrote as  $4\frac{2}{1}, 4\frac{2}{2}, 4\frac{2}{3}, 4\frac{2}{4}, \dots$  then a number between 1 and 6 must be found by the use of a multiplier greater than  $4\frac{2}{1}$ , which was impossible.

3. That induction as used by Wallis was not a satisfactory method of proof (a reservation already expressed by both Huygens and Hobbes).
4. That the result for the sum of an arithmetic progression was not restricted to those progressions where the difference was equal to 1.

Wallis answered all these points to his own satisfaction in a long letter completed in November 1657.<sup>635</sup> The details of the subsequent argument need not concern us here: what was at stake was perhaps not so much the mathematics as national honour and personal pride. Fermat's failure to publish meant that he now suffered the unhappiness of seeing Wallis acclaimed instead of him. Wallis was never afraid of engaging in controversy but in this case it was Fermat who began the argument and kept it fuelled, and who introduced the overt nationalism which came to embitter all Wallis's later dealings with French mathematicians. 'It is not that I mean by this to renew the jousts and ancient tiltings of lances which the English once carried out against the French', wrote Fermat somewhat mendaciously in August 1657.<sup>636</sup>

Like Hobbes, Fermat attacked the foundations of Wallis's approach: symbolism and induction. When the correspondence came to an end the following year, Fermat (by now writing in Latin) was still holding out for classical geometric methods:<sup>637</sup>

*Monemus ut sepositis tantisper speciebus Analyseos . .*

We advise that you would lay aside (for some time at least) the Notes, Symbols, or Analytick Species (now since Vieta's time, in frequent use,) in the construction and demonstration of Geometrick Problems, and perform them in such method as Euclide and Apollonius were wont to do; that the neatness and

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<sup>635</sup> Wallis to Fermat 21 November 1657, *ibid.* no. 16.

<sup>636</sup> Fermat to Digby 15 August 1657, *ibid.* no. 12.

<sup>637</sup> Fermat to Digby June 1658, *ibid.* no. 46, translated in *Treatise of algebra*, 305.

elegance of Construction and Demonstration, by them so much affected, do not by degrees grow into disuse.

Wallis argued, however, that his intention was not to abandon the traditional methods but to show how they might be improved and extended.<sup>638</sup>

To the elegance and neatness of the Ancients way of Construction and Demonstration, I am no Enemy. And that these Propositions might be so demonstrated, I was so far from being ignorant, that I had again and again affirmed it; but had shewed also the reason why I chose to go a shorter way. . . because by this means I might in a compendious [concise] continued discourse deliver that in brief, which in the other way must (with more pomp and solemnity) be parcelled out into several Lemmas, and preparatory Propositions. . . But [Fermat] doth wholly mistake the design of that Treatise; which was not so much to shew a Method of Demonstrating things already known; (which the Method that he commends, doth chiefly aim at,) as to shew a way of *Investigation* or finding out of things yet unknown: (Which the Ancients did studiously conceal.) . . And that therefore I rather deserved thanks, than blame, when I did not only prove to be true what I had found out; but shewed also, how I found it, and how others might (by those Methods) find the like.

The dispute between Wallis and Fermat, like that between Wallis and Hobbes, seems to reflect an uneasy transition from old paradigms to new, from the Classical, geometric and synthetic to the modern, symbolic and analytic, but in the case of Fermat the matter was rather more complex: for all his avowed disdain for ‘Analytick Species’, Fermat’s mathematics was deeply rooted in the analytic methods of Viète and he repeatedly went far beyond the Classical foundations he ostensibly espoused.<sup>639</sup> The real tension between Wallis and Fermat was perhaps not so much in their differences of approach as in the similarities of achievement. Many of Wallis’s results had been foreshadowed by Fermat, albeit in different language, many years before (some will be mentioned in more detail below). Fermat, having failed to publish or otherwise disseminate his results, could hardly

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<sup>638</sup> *Treatise of algebra*, 305-306.

<sup>639</sup> See Mahoney 1973, 26-71.

blame Wallis for assuming priority, but challenged him instead on his methods and foundations. Wallis was unshaken: for him the correctness of his results was justification enough.

The *Arithmetica infinitorum* did at least prompt Fermat, for the first time, to take English mathematicians seriously, and his subsequent correspondence with Wallis and Brouncker on problems of number theory is described in detail in Chapter 8 of this thesis. The later exchanges, however, like those around the *Arithmetica infinitorum*, left both sides feeling aggrieved and perhaps did lasting damage in beginning to change Wallis's perception of French mathematicians for ever for the worse.

Three later reactions to Wallis's *Arithmetica infinitorum* should perhaps be mentioned here as evidence of continued interest in the book towards the end of the seventeenth century and beyond. In 1682 the astronomer Ismael Boulliau, a longstanding acquaintance of Wallis, published his *Opus novum ad arithmetica infinitorum* in which he expanded at considerable length on the *Arithmetica infinitorum* up to Proposition 109 but left untouched the more difficult later material, thus doing for the *Arithmetica infinitorum* exactly what Gilbert Clark had done for Oughtred's *Clavis*.<sup>640</sup> Wallis, in *A treatise of algebra*, noted that Boulliau found the work 'sound and good', ('Only he thinks I have not done my invention so much honour as it doth deserve').<sup>641</sup> Five years after Wallis's death, David Gregory, Savilian Professor of Astronomy, wrote a summary of Wallis's life and work, and claimed that 'the *Arithmetica infinitorum* has ever been acknowledged to be the foundation of all the Improvements that have been made in Geometry since that time,'<sup>642</sup> an opinion which Wallis himself would have shared.

A fitting closing comment on the *Arithmetica infinitorum* is to be found a century later in Charles Babbage's unpublished essay 'Of induction' written in

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<sup>640</sup> Boulliau 1682; Clark 1682. Although both commentaries were published in 1682 both had been many years in the writing. Both were reviewed in the *Acta eruditorum* (see Bibliography).

<sup>641</sup> *Treatise of algebra*, 310-311.

<sup>642</sup> MS Smith 31, f. 58.

1821.<sup>643</sup> The concept of induction which had so troubled Wallis's contemporaries was by this time seen by Babbage as an essential feature of modern scientific thought:

Few works afford so many examples of pure and unmixed induction as the *Arithmetica infinitorum* of Wallis and although more rigid methods of demonstration have been substituted by modern writers this most original production will never cease to be examined with attention by those who interest themselves in the history of analytical science or in examining those trains of thought which have contributed to its perfection.

### **The enlargement of the mathematical empire**

The criticisms of Hobbes and Fermat failed to deter Wallis, or others, from building on the ideas of the *Arithmetica infinitorum*. Over the next ten years or so new results on quadrature, rectification, and infinite series began to proliferate, many of them directly inspired by Wallis's work. In addition the book made easily available, sometimes for the first time, algebraic formulations of common curves, formulae for figurate numbers, algebraic notation for continued fractions, and subscript notation for lengthy or infinite sequences. In each of these areas the *Arithmetica infinitorum* made a lasting contribution to a new kind of algebra, an algebra that was no longer just a convenient shorthand for solving equations but an evolving language in which all mathematical thought was beginning to find expression.

The most important developments which appeared either in the book itself or which arose directly from it will be described under the following headings: *Generalised algebraic formulae, Continued fractions, Algebraic formulation of conics, Rectification, Quadrature, The general binomial theorem, The extension of the number system.*

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<sup>643</sup> British Library Add MS 37202. See Dubbey 1978, 109-114.



### *Generalised algebraic formulae*

The formulae for the sums of integers, squares, cubes and so on in the early propositions of the *Arithmetica infinitorum* have already been noted. Later, in the course of interpolating his tables, Wallis also needed formulae for triangular, pyramidal and higher triangular polyhedral numbers. Somewhat surprisingly, he treated the triangular numbers themselves not as the sums of consecutive integers, for which he already had a formula, but as a physical configuration of points on a triangular lattice. Beyond that, however, he abandoned any physical interpretation and worked out the formulae for the higher numbers by repeated addition up to the fourth dimension (the triangu-li-triangulars) and then by multiplication to the seventh dimension (the triangu-li-triangu-li-pyramidals) for which the formula is:

$$\frac{l^7 + 21l^6 + 175l^5 + 735l^4 + 1624l^3 + 1764l^2 + 720l}{5040}$$

Wallis was not the first to arrive at these results: Harriot had written them out almost half a century earlier in his treatise on figurate numbers, and he too had gone as far as the seventh dimension.<sup>644</sup> It is an open question how much Wallis in the early 1650s knew of Harriot's work. Harriot's treatise had been copied in its entirety by Charles Cavendish<sup>645</sup> and parts of it appear throughout the mathematical papers of Pell, but whether Wallis and Pell discussed it before 1655 is not known. For the most part Wallis's method was considerably longer and very much more forced than Harriot's: he used six closely printed pages<sup>646</sup> to arrive at the formulae Harriot had found in less than a single side, but right at the end his method became remarkably similar to Harriot's. Could he have known something, even indirectly, from Pell? Or did he rediscover the same formulae for himself?

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<sup>644</sup> Harriot, 'De numeris triangularibus et inde de progressionibus arithmetice magisteria magna', undated, British Library Add MS 6782 ff. 107-146; 108-109. Folio 108 is reproduced in Lohne 1979, 294. Where Wallis later wrote  $l$  or  $l^3$  Harriot used  $n$ ,  $nnn$  etc, but otherwise the formulae are identical.

<sup>645</sup> British Library Harley MS 6083, ff. 403-455.

<sup>646</sup> *Arithmetica infinitorum*, Propositions 171-182.

Fermat too knew and used the results for the triangular polyhedral numbers as early as 1636 but expressed them verbally.<sup>647</sup> It seems that Fermat used these results in turn to derive formulae for sums of powers, whereas Wallis worked in the opposite direction.<sup>648</sup> For Fermat, as for Wallis, however, such formulae had opened the door to quadrature;<sup>649</sup> no wonder Fermat was less than happy when Wallis, twenty years later, published as though his methods were entirely new.

Mathematically Wallis went beyond Harriot or Fermat in many ways: he extended his sums of powers to fractional and negative powers, and he took the even bolder step of interpolating non-integers between the figurate numbers. But perhaps just as important was that Wallis, unlike Harriot or Fermat, had a strong sense of the value of publishing mathematical results. The *Arithmetica infinitorum* demonstrated for all to see that arithmetic relationships could be expressed in general algebraic form and, further, that new relationships could be revealed by the correct manipulation of symbols. When Newton and Mercator a few years later handled symbolic expressions by the laws of arithmetic<sup>650</sup> they were following a precedent laid down in the pages of the *Arithmetica infinitorum*.

### *Continued fractions*

Wallis's infinite fraction for  $\pi$  was afterwards converted by Brouncker into what is now called a continued fraction (the name comes from Wallis's description of the denominator as *continue fractum* or 'continually broken'). This work is described in detail in Chapter 8 of this thesis, but it may be noted here that this was the first time such fractions had arisen in English mathematics. Further, Wallis worked with such fractions not only arithmetically but algebraically and introduced subscript notation  $N_1, N_2, N_3$  and  $D_1, D_2, D_3$  for handling successive

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<sup>647</sup> Fermat to Roberval 4 November 1636, quoted in Mahoney 1973, 230-231.

<sup>648</sup> Fermat had results for cases where the power is an integer or the reciprocal of an integer but not, as Wallis did, for general fractional powers. Mahoney 1973, 231-233 and 238-239.

<sup>649</sup> Mahoney 1973, 233-239.

<sup>650</sup> Mercator 1668, Proposition XV; Newton 1669, 212-215.

numerators and denominators. Such notation was another innovation and a major step forward in handling lengthy or infinite sequences of all kinds.<sup>651</sup>

### *Algebraic formulation of conics*

In the same year that Wallis published his *Arithmetica infinitorum*, he also brought out his *De sectionibus conicis*, the first systematic algebraic treatment of conics.<sup>652</sup> Descartes' algebraic treatment of curves had been published in French in 1637 and translated into Latin in 1649, and was the inspiration for much new continental mathematics during the 1650s. Wallis, however, was pursuing not so much Descartes' agenda as his own. He devised his algebraic treatment in his own style: his abbreviations, unlike Descartes', were tied to the geometrical properties of the curves, *d* for diameter, *l* for *latus rectum*, and so on. He was also working with his own ends in mind: to further the methods of the *Arithmetica infinitorum*, which could only be applied after the parameters of the curves had been related algebraically.

The close connection between *De sectionibus conicis* and *Arithmetica infinitorum* is superficially evident from the fact that the two books were printed side by side in both sets of Wallis's collected works.<sup>653</sup> It is also explicit in the texts themselves: *De sectionibus conicis* begins exactly as the *Arithmetica infinitorum* does, with the area of a triangle, proved in the same way, and ends by promising a series of results on areas and volumes bounded by curves, a promise specifically taken up in the *Arithmetica infinitorum*.<sup>654</sup> Much later Wallis himself described the relationship between the two works:<sup>655</sup>

Consonant to the Doctrines here delivered, I have in a short Treatise (published together with my Arithmetick of Infinites) given a compendious and clear

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<sup>651</sup> The introduction of subscript notation is not mentioned in Cajori 1928-29.

<sup>652</sup> Wallis 1655a. Like the *Arithmetica infinitorum* itself, Wallis's *De sectionibus conicis* is excluded from Rider 1982.

<sup>653</sup> Wallis 1656; Wallis 1695, 291-478.

<sup>654</sup> *De sectionibus conicis*, Proposition 48; *Arithmetica infinitorum*, proposition 45.

<sup>655</sup> *Treatise of algebra*, 290, 292.

account of the Doctrine of *Conick Sections* (as they are wont to be called) . . . whereas I find some others (to make it look, I suppose, the more Geometrical) to affect Lines and Figures; I choose rather to demonstrate universally from the nature of Proportions and regular Progressions; because such Arithmetical Demonstrations are more Abstract, and therefore more universally applicable to particular occasions. Which is one main design that I aimed at in this Arithmetic of Infinites.

The treatise on conics, therefore, both arose from, and was an essential precursor to, the *Arithmetica infinitorum*, and the two books should be seen as belonging inseparably together.

### *Rectification*

Early on in the *Arithmetica infinitorum* Wallis outlined a possible method for rectifying (determining the length of) a parabola.<sup>656</sup> Suppose, using modern conventions, that the parabola is symmetrical about the  $y$  axis, with vertex at the origin, and take ordinates at equal intervals  $a$  along the  $x$  axis. The corresponding  $y$  values, suitably scaled, will be 1, 4, 9, 16, . . . with differences 1, 3, 5, 7, . . . and so (by simple application of Pythagoras' theorem) the lengths of curve cut by the ordinates will be, said Wallis,  $\sqrt{(a^2 + 1)}$ ,  $\sqrt{(a^2 + 9)}$ ,  $\sqrt{(a^2 + 25)}$ , . . . The length of any portion of the curve can therefore be found by summing such a sequence for sufficiently small  $a$ . The sum itself was out of Wallis's reach (it was essentially the same problem as finding  $\sum_r \sqrt{(1 - r^2)}$  for the area of the circle) but the importance of his insight was that it reduced the geometric problem of rectification to the summing of a number sequence.

In 1657 William Neile (1637-1670), then a young student at Oxford, showed how to rectify the curve whose modern equation is  $y^2 = kx^3$ . His method was based on summation of indivisibles but, unlike Wallis, he used a purely geometric approach. In *A treatise of algebra* Wallis claimed that he had foreseen the possibility of rectifying just such a curve but that Neile had taken up his hints

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<sup>656</sup> *Arithmetica infinitorum*, Proposition 38.

before he had time to pursue them himself. This was perhaps a case of being wise after the event: Wallis had certainly suggested a procedure for rectification and had listed some of the higher parabolas which might be amenable to his method, but Neile's curve was not one of them, and Neile's method was very different from Wallis's. Nevertheless, Wallis immediately saw how to re-write it algebraically using the methods of *De sectionibus conicis* and the *Arithmetica infinitorum*, and it was Wallis who named the curve the 'semicubical parabola'. It was also Wallis who published Neile's method with full attribution, alongside his own modification of it in his treatise on the cycloid in 1659.<sup>657</sup>

The rectification of curves was an idea whose time had come. Hendrick Van Heuraet in Holland independently worked out the same result for the semicubical parabola and that too was published in 1659, in Frans van Schooten's new edition of Descartes' *Géométrie*.<sup>658</sup> That same year Fermat also wrote a treatise on rectification in which he solved the problem for both the semicubical parabola and the cycloid. It was printed in 1660, the only work he published in his lifetime.<sup>659</sup> Wallis later claimed that all these methods sprang directly or indirectly from his own:<sup>660</sup>

And I do not at all doubt that this notion there hinted, gave the occasion (not to Mr Neil only, but) to all those others (mediately or immediately,) who have since attempted such Rectification of Curves (nothing in that way having been attempted before;) and even to that of Mons. Hugens (which he thinks did give the occasion to Mons. Heurats invention) giving the Curve surface of a Parabolick Conoid, equal to a Circle . .

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<sup>657</sup> Wallis 1695, 551-554. In his biographical notes on Neile wrote: 'Enquire of Dr Wallis of his [Neile's] rare invention printed in one of his bookes: never before found out by man', Aubrey 1898, II, 94.

<sup>658</sup> Van Heuraet 1659; Van Maanen 1984.

<sup>659</sup> Fermat 1660. Fermat's 'De linearum curvarum' was heavily annotated by Wallis in his own copy of Fermat 1679 (Bodleian Library, Savile B.7). For a detailed discussion of Fermat's methods of rectification see Mahoney 1973, 267-281.

<sup>660</sup> *Treatise of algebra*, 298, 297 respectively.

I will not disparage Mons. Fermat's Invention herein, nor his Demonstrations thereof. But allow the Invention to be very Ingenious, and his Demonstrations to be good and full. . . Nor will I impute it as a fault in him that others had done the same thing before him: Or that he had (or might have had) the first hints of it from the Arithmetick of Infinites, (which I am sure he had read.) . .

With regard to Van Heuraet and Huygens, Wallis was overestimating his own importance: Van Heuraet's method, like most Dutch mathematics at the time, was firmly rooted in the work of Descartes rather than anything learned from Wallis. The publication of Neile's method in 1659 appeared to settle for the time being the question of priority between Van Heuraet and Neile, but the dispute flared again in 1673, at which point Wallis again took up the argument on Neile's behalf and persuaded both Brouncker and Wren to support him.<sup>661</sup> Such heavy opposition was in vain since Van Heuraet's work appears to have been independent of anything done in England.

Fermat, in Toulouse and increasingly isolated, had apparently heard of neither Van Heuraet's result nor Neile's. The first sentence of his *De linearum curvarum* read: 'Never, so far as I know, have mathematicians compared straight lines to purely geometrical curved lines'. He was prompted to write his treatise following a reference in Pascal's *Lettres de A. Dettonville*<sup>662</sup> to Christopher Wren's 1658 rectification of the cycloid (which Fermat would not have regarded as a *geometrical curve*).<sup>663</sup> Fermat approached the problem in the 'regular Archimedean way', a refutation rather than an endorsement of Wallis's new

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<sup>661</sup> Wallis 1673. The letter from Brouncker, preserved by the Royal Society, is in Wallis's hand, see Hall and Hall 1965-86, X, 291-292. I have recently discovered a draft of the letter from Wren, also in Wallis's hand, among the papers of Pell in British Library Add MS 4428, f. 314.

<sup>662</sup> Pascal 1659, 90f.

<sup>663</sup> Descartes defined geometric curves as those whose relationship to the axis could be expressed by a single equation, while non-geometric curves such as the spiral or quadratrix required more complex rules for their generation, see Fauvel and Gray, 344-345. Fermat, using a similar distinction, discounted the cycloid as a 'geometrical curve', thus conveniently invalidating Wren's rectification. For further discussion on the construction and classification of curves see Bos 1993, 23-36.

methods. Wallis's remark that he would not 'impute it as a fault in him [Fermat] that others had done the same thing before him' has perhaps to be seen as an echo and a riposte to similar remarks made by Fermat in 1657.<sup>664</sup>

I have read the *Arithmetica infinitorum* of Wallis and have great regard for its author . . . who no doubt did not know that I had pre-empted his work.

The successes of Neile, Van Heuraet and Fermat with rectification between 1657 and 1659 appear to have been independent of each other and to have owed little or nothing to Wallis. The *Arithmetica infinitorum* (together with *De sectionibus conicis*), however, showed how the results could be achieved and written algebraically, and for this Wallis could rightly claim credit. In fact it was Brouncker who went furthest in this respect and produced the first rectification of a geometric curve in modern algebraic notation. His work was published alongside that of Neile and Wallis in *De cycloide* and is discussed more fully in Chapter 8 of this thesis.

### *Quadrature*

The primary aim of the *Arithmetica infinitorum* was to devise general methods for the quadrature of curves and there were, as already noted, numerous examples throughout the text. The appearance of the *Arithmetica infinitorum* provoked Fermat to set out his own results, some obtained up to twenty years earlier. His treatise on the subject was almost certainly written in 1658 or 1659 but remained unpublished until 1679, fourteen years after his death.<sup>665</sup> By then, work that had been ground-breaking in the 1630s was no longer new and never gained the appreciation or influence it had deserved.

In England Wallis's work had more lasting repercussions. One curve he had not been able to deal with was the rectangular hyperbola, in modern notation  $y = 1/x$ , since his rule for summing  $n$ th powers involved the ratio  $1/(n+1)$  and therefore broke down for  $n = -1$ . Nicolaus Mercator, a Danish mathematician who

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<sup>664</sup> Fermat to Digby 20 April 1657, in Wallis 1658, no. 4.

had settled in England in 1653, overcame the problem by what amounted to a change of axes so that the equation became  $y = 1/(1+x)$ . By long division Mercator obtained

$$y = \frac{1}{1+x} = 1 - x + x^2 - x^3 + \dots$$

and then used Wallis's methods to sum powers of  $x$  across a series of strips to find areas bounded by the curve and the  $x$  axis. It was already known that such areas obeyed the fundamental properties of logarithms. Hence Mercator deduced the relationship:

$$\ln(1+x) = x - \frac{x^2}{2} + \frac{x^3}{3} - \dots$$

Mercator's work was published in his *Logarithmotechnia* in 1668 and Wallis wrote an account of it for the *Philosophical transactions* later the same year.<sup>666</sup>

The publication of Mercator's results prompted two other mathematicians to reveal their work. First, Brouncker published his quadrature of the hyperbola.<sup>667</sup> His method devised at least twelve years earlier, filled the required space with a cleverly chosen (fractal) sequence of decreasing rectangles, and is described more fully elsewhere.<sup>668</sup> Second, Isaac Newton hurriedly composed his *De analysi* to show that he too had independently arrived at the same result as Mercator.<sup>669</sup> In *De analysi* Newton, like Mercator, obtained the infinite series for  $(1+x)^{-1}$  by straightforward long division, but four years earlier he had arrived at it by a different method which was deeply rooted in Wallis's work, and will be discussed now in connection with the binomial theorem.

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<sup>665</sup> Fermat 1658/9; Fermat 1679, 44-57. For full discussion of Fermat's work on quadratures see Mahoney 1973, 244-267.

<sup>666</sup> Wallis 1668. Wallis reported the contents of Mercator's book without judgement as to its merit.

<sup>667</sup> Brouncker 1668.

<sup>668</sup> Coolidge 1949, 141-146; Stedall 2000c.

<sup>669</sup> Newton's 'De analysi' was composed in 1669 but was unpublished until it appeared in Newton 1711.



### *The general binomial theorem*

Newton's story is best introduced in his own words:<sup>670</sup>

In the winter between the years 1664 and 1665 upon reading Dr Wallis's *Arithmetica infinitorum* and trying to interpolate his progressions for squaring the circle, I found out first an infinite series for squaring the circle and then another infinite series for squaring the Hyperbola . .

Newton's manuscripts confirm his account. His 1664 notes on the *Arithmetica infinitorum* are followed without a break by of his own investigations.<sup>671</sup> As Whiteside, his editor and interpreter, has observed: 'with the Wallis notes, there is no true dividing line between the summarized impact of the original and the following wave of new ideas'.<sup>672</sup>

Almost immediately, however, there was a crucial difference between Newton's work and Wallis's. From the start Newton wrote his sequences in terms of the variable  $x$ , and made the coefficients of each term  $x^n$  distinct, whereas in Wallis's work the coefficients had been absorbed in a single numerical sum which hid their individual contributions. Newton's advance has been described by later authors as 'freeing the upper bound' of the integral  $\int_0^1 (1-r^2)^{1/2} dr$  to give the

more general  $\int_0^x (1-r^2)^{1/2} dr$ .<sup>673</sup> Neither Wallis nor Newton, however, would have

understood this notation or terminology, and it is perhaps more helpful to consider the change from a seventeenth-century perspective. Wallis was interested only in the whole quadrant<sup>674</sup> and, following Cavalieri, wanted the numerical ratio of the quadrant to the circumscribing square. In other words, Wallis posed the problem

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<sup>670</sup> Cambridge University Library, MS Add 3968.41, f. 76.

<sup>671</sup> Newton 1664.

<sup>672</sup> Whiteside 1967-81, I, 13.

<sup>673</sup> Whiteside 1967-81, I, 106; Cohen 1974, 46.

<sup>674</sup> Leibniz pointed out this limitation of Wallis's method after the *Arithmetica infinitorum* was republished in Wallis 1695, see Leibniz 1696; Whiteside 1961a, 322-324.

in such a way that his variable parameters neatly cancelled out to leave him with a numerical answer. Newton set himself the more difficult task of finding partial areas, which forced him to work with a variable abscissa,  $x$ , and hence, eventually, to move away from ratios to absolute areas calculated in terms of  $x$ . The conceptual shift was therefore not to do with ‘upper bounds’, but from whole to partial areas, and away from the ideas of ratio which had so pervaded Greek and early European mathematics. The importance of this can hardly be overestimated: it opened the way to seeing areas (and associated logarithmic and trigonometrical quantities) as functions of a free variable, and to the expression of such functions as infinite series.

In all other ways Newton followed Wallis closely. He had no difficulty, using Wallis’s interpolated values, in finding the infinite series for the part area of a

quadrant, in modern notation  $\int_0^x (1-r^2)^{1/2} dr$ , as:

$$x - \frac{x^3}{6} - \frac{x^5}{40} - \frac{x^7}{102} - \frac{5x^9}{1152} - \dots$$

Newton took from Wallis not only his numerical coefficients but also the multiplicative method of generating them. Whiteside has remarked that Newton’s use of  $\frac{0}{0}$  was ‘a typically Wallisian flourish’ but it was more than a mere flourish.

When Newton wrote

$$\frac{0 \times 1 \times -1 \times 3 \times -5 \times 7 \times -9 \times 11}{0 \times 2 \times 4 \times 6 \times 8 \times 10 \times 12 \times 14}$$

he was following Wallis’s precepts exactly.

When Newton came to consider the area under a hyperbola,  $(\int_0^x (1+r)^{-1} dr)$

he extrapolated Wallis’s table by a different method. He set out the coefficients of

$\frac{x^n}{n}$  in  $\int_0^x (1+r)^n dr$  in the form shown below (Table V) and then used the fact that

each term is the sum of the figure to the left of it and the one above that to fill in the column for  $n = -1$ .

Table V. Newton's extrapolation of Wallis's table.

	-1	0	1	2	3	4	5	6	7
$x$	1	1	1	1	1	1	1	1	1
$x^2/2$	-1	0	1	2	3	4	5	6	7
$x^3/3$	1	0	0	1	3	6	10	15	21
$x^4/4$	-1	0	0	0	1	4	10	20	35
$x^5/5$	1	0	0	0	0	1	5	15	35
$x^6/6$	-1	0	0	0	0	0	1	6	21
$x^7/7$	1	0	0	0	0	0	0	1	7

This gave Newton the area under the hyperbola  $y = (1 + x)^{-1}$  as:

$$x - \frac{x^2}{2} + \frac{x^3}{3} - \frac{x^4}{4} + \dots$$

Newton returned to this work in the autumn of 1665 and re-wrote it in more finished form.<sup>675</sup> Now, however, he found a new method of interpolating Wallis's table. Wallis, as we have seen, used multiplication to obtain each entry from a previous one, but Newton saw a simpler method based on addition. Newton's inspiration arose, perhaps, from his earlier extension of the table leftwards for the hyperbola: he did not say. He noted, however, that starting from any column of Table V, the rows take the pattern:

$$\begin{array}{cccccc} a & a & a & a & a & \dots \\ b & b+c & b+2c & b+3c & b+4c & \dots \\ d & d+e & d+2e+f & d+3e+3f & d+4e+6f & \dots \end{array}$$

So, for instance, the third row is generated from the first zero onwards by putting  $d = 0, e = 0, f = 1$ , but from 1 onwards by putting  $d = 1, e = 2, f = 1$ . Newton, like Wallis, made the bold but correct assumption of continuity between the values in the table, and just as Wallis had done before him, he allowed for intermediate entries between the integers in each row, which he denoted by the single symbol \* (which could assume different values in different cells).

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<sup>675</sup> See Newton 1665; Whiteside 1961c; Dennis and Confrey 1996.

	0		1		2		3		4		5
x	1	*	1	*	1	*	1	*	1	*	1
$x^2/2$	0	*	1	*	2	*	3	*	4	*	5
$x^3/3$	0	*	0	*	1	*	3	*	6	*	10
$x^4/4$	0	*	0	*	0	*	1	*	4	*	10
$x^5/5$	0	*	0	*	0	*	0	*	1	*	5
$x^6/6$	0	*	0	*	0	*	0	*	0	*	1
$x^7/7$	0	*	0	*	0	*	0	*	0	*	0

From the pattern established for the third row Newton could write:

$$d = 0$$

$$d + e = *$$

$$d + 2e + f = 0$$

$$d + 3e + 3f = *$$

$$d + 4e + 6f = 1$$

$$d + 5e + 10f = *$$

$$d + 6e + 15f = 3$$

The equations not involving \* are mutually consistent and were easily solved to give  $d = 0$ ,  $e = -\frac{1}{8}$ ,  $f = \frac{1}{4}$  from which Newton could calculate successive values of \* as  $-\frac{1}{8}$ ,  $\frac{3}{8}$ ,  $\frac{15}{8}$ , . . . just as Wallis had found. Newton's new method, however, enabled him to go beyond Wallis for he could extend his method to allow for not just one but two (or more) intermediate terms. This gave him the coefficients of  $(1+x)^{p/q}$  for any integers  $p, q$  without difficulty.

Newton's method requires some comment here. His assumption of continuity was justified because he was essentially fitting a polynomial curve to each row of the table by a method of constant differences (a method he was to develop more explicitly in later years).<sup>676</sup> Harriot had worked out the constant difference properties of figurate numbers many years earlier, and following his lead both Warner and Pell took up constant difference methods for the interpolation of tables. The pattern set out by Newton is to be found in the unpublished

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<sup>676</sup> Newton 1675-76, 52-69.

manuscripts of all three,<sup>677</sup> but Newton as a young student in 1665 was unlikely to have seen them. The method was also well known to Henry Briggs who wrote extensively on subtabulation in the introduction to his *Arithmetica logarithmica* in 1624.<sup>678</sup> The evidence for Newton having read Briggs, however, is inconclusive and the possibility must remain that Newton reinvented the method for himself.<sup>679</sup>

Finally, Newton went back to Wallis for the last time. To find the general form of his new coefficients he once again used Wallis's multiplicative pattern and wrote the general term as:<sup>680</sup>

$$\frac{p}{q} \times \frac{p-q}{2q} \times \frac{p-2q}{3q} \times \frac{p-3q}{4q} \times \dots$$

Putting  $m = p/q$ , this simplifies to the general binomial coefficient:

$$m \times \frac{m-1}{2} \times \frac{m-2}{3} \times \frac{m-3}{4} \times \dots$$

This was the form in which Newton presented his result in the famous *Epistola posterior* to Oldenburg in October 1676.<sup>681</sup> Harriot had written the identical formula half a century earlier for the case when  $m$  is an integer; Newton's genius was to extend the formula to both negative and fractional indices.<sup>682</sup> Wallis readily

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<sup>677</sup> See, for example: British Library Add MS 6782, ff. 112, 116 (Harriot); Add MS 4396, f. 77 (Warner); Add MS 4415, f. 113 (Pell). The latter, a long folded sheet, contains an exceptionally long constant difference table in 60 rows and 4 columns. Numerous smaller examples are to be found throughout Pell's papers.

<sup>678</sup> Briggs 1624; Whiteside 1961b.

<sup>679</sup> Fraser 1927, 58, suggested that Newton drew his ideas from Briggs but Whiteside 1967-81, I, 13 n. 32, regards his evidence as 'flimsy and circumstantial'.

<sup>680</sup> Newton actually used  $x$  and  $y$  as his integers.

<sup>681</sup> Newton to Oldenburg 24 October 1676, nos. 188, 189 in Turnbull 1959-77, II, 110-163.

<sup>682</sup> British Library Add MS 6782, f. 110. Harriot, like Newton, had extended his tables to include negative values of  $m$ , see Add MS 6782, f. 330 reproduced in Lohne 1979, 294. Harriot's formula, however, in keeping with the contemporary understanding of figurate numbers, was intended only for positive values of  $m$ .

recognised that Newton had surpassed him and was the first to publish Newton's results.<sup>683</sup>

As Newton's work progressed he, like Wallis, had moved increasingly away from concepts of area into purely numerical interpolation. Both he and Wallis searched the empty spaces between the numbers in Wallis's table and emerged with rich treasures. Newton, however, went much further in his use of algebraic generalisation and in doing so began to write all mathematics in a new language. When Newton in 1666 showed how an angle could be found from its sine as:

$$\arcsin x = x + \frac{x^3}{6} + \frac{3x^5}{40} + \frac{5x^7}{112} + \dots$$

he was not just producing a brilliant new result but changing the way mathematics was written and conceived. For both Wallis and Newton arithmetic provided the essential underpinning of this new work (and Newton continually checked that his algebra did not contravene the laws of arithmetic) but Newton went far beyond Wallis in using algebra as a language, and in doing so opened the way forward to modern mathematics. Newton more than anyone consolidated the enlargement of the mathematical empire that Wallis had begun, but his debt to the *Arithmetica infinitorum* was immense.

#### *The extension of the number system*

In one overlooked respect Wallis remained far ahead of any mathematician of the mid seventeenth century. Sandwiched between Propositions 190 and 191 of the *Arithmetica infinitorum* is a *Scholium* (commentary) which deserves more recognition than it has received, in which Wallis discussed the kind of number he needed to complete the elusive quadrature of the circle. By this time, Wallis realised that the numbers so far known, even surds, were inadequate for his purpose, and that something completely new was required. In a passage remarkable for its time, Wallis argued that mathematicians had repeatedly

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<sup>683</sup>*Treatise of algebra*, 330-346. Wallis published the *Epistola prior* almost in its entirety together with some supporting material from the *Epistola posterior*. Turnbull 1959-77, III, 220 n. 4, is inaccurate on this point.

introduced such new numbers to serve new purposes: to subtract greater quantities from lesser (apparently impossible) they used negative numbers; to denote division of numbers by non-factors (also apparently impossible) they used rationals, for example,  $\frac{3}{2}$ ; to denote square roots of non-squares, they used surds.<sup>684</sup> Cube (or higher) roots were a further useful extension, and the use of  $n^{\text{th}}$  roots allowed interpolation of one or more mean proportionals between any two terms in a geometric progression (for example, between 3 and 6 there was the single term  $\sqrt{3 \times 6}$ , or a pair of terms  $\sqrt[3]{3 \times 3 \times 6}$ ,  $\sqrt[3]{3 \times 6 \times 6}$ ). What Wallis needed, arising from Proposition 185, was a more difficult interpolation, between the terms of the series 1,  $\frac{3}{2}$ ,  $\frac{15}{8}$ ,  $\frac{105}{48}$ , . . . where the successive multipliers were themselves increasing ( $\frac{3}{2}$ ,  $\frac{5}{4}$ ,  $\frac{7}{6}$ , . . .). Wallis called such a sequence ‘hypergeometric’ and introduced the symbol  $\eta$  (borrowed from Oughtred)<sup>685</sup> to denote an interpolated mean. In his notation:

$$\square = \eta : 1 | \frac{3}{2} :$$

Wallis recognised that such a number could not be found exactly, but nor, he pointed out could a surd such as  $\sqrt{18}$ . In both cases, however, it was possible to approximate the true value to any required degree of accuracy, and for Wallis, following Euclid, the possibility of such approximation implied equality. Further, just as surds could be handled by the usual operations of arithmetic, so could these new numbers (though Wallis declined to go into detail here). In short, Wallis was introducing not just a new *kind* of number but a new definition of the very *nature* of number: new numbers could, and indeed must, be introduced to allow the completion of any properly defined arithmetic process. That such numbers could be evaluated as accurately as one chose and satisfied the usual laws of arithmetic was sufficient.

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<sup>684</sup> There is no reference to this discussion in Pycior 1997 even though the development of concepts of number in early algebra is a central theme of the book. Pycior, 125, briefly mentions the later, related, discussion in *A treatise of algebra* but misses the main purpose of it which was to go *beyond* numbers already known.

<sup>685</sup> Oughtred 1648, 166.

In *A treatise of algebra* twenty years later, Wallis returned to this discussion and explained the use of negatives, rationals and surds as before.<sup>686</sup> Now, however, he added some new thoughts on equations and pointed out that the full solution of finite equations required yet another kind of number, those ‘(commonly called) Imaginary’. The quadrature of the circle, however, forced the mathematician beyond finite equations to expression such as  $(1-x)^{1/2}$ ,  $(1-x)^{3/2}$ , which Wallis described as being ‘Intermediate . . . between the Lateral and Quadratick; or between the Quadratick and the Cubick’. Following Newton, who in *De analysi* used the term *aequationes infinitas* (‘infinite equations’) for infinite (convergent) series,<sup>687</sup> Wallis described such expressions as ‘Equations’ and saw that they could lead to numbers beyond those required for finite equations.<sup>688</sup>

There must be some other way of Notation invented, (if we would express it in Numbers,) than either Negatives or Fractions; or (what are commonly called) Surd Roots, or the Roots of Ordinary Equations; or even the Imaginary Roots of such Impossible Equations in the ordinary forms; even such as shall denote the Root of such intermediate Equations between the Ordinaries.

Only fifty years after Descartes described negative roots as ‘false’ Wallis was feeling his way towards the later distinction between *algebraic* numbers which satisfy ‘Ordinary Equations’ and *transcendental* numbers, those which no ‘Ordinary Equation’ can define. For Wallis the justification for extending the number system this far lay in the laws of arithmetic: the necessary completeness of those laws not only allowed but actually demanded new kinds of number. No modern mathematician could disagree. Wallis, the Great Interpolator, not only filled the spaces of his tables, but ventured further than anyone in else in his time into the interstices of the number system itself.

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<sup>686</sup> *Treatise of algebra*, 315-317.

<sup>687</sup> Whiteside 1967-81, II, 240-241, n. 127.

<sup>688</sup> *Treatise of algebra*, 317.



## Conclusion

Perhaps more any previous mathematical text (and few since) the *Arithmetica infinitorum* was a record of ‘work in progress’. The book gives the impression of having been written over months, even years, and each new idea is explored at length until a fresh insight emerges to carry the work forward into a new phase. Wallis could have condensed his findings into a more polished and very much shorter book; instead he chose to take his reader with him on his journey of exploration, and to share his own astonishment at where in the end he found himself. This inside view of a major piece of mathematical invention makes the *Arithmetica infinitorum* even now a valuable text for students of mathematics.

The *Arithmetica infinitorum* stood both mathematically and chronologically at the mid point of the seventeenth century. In the first energetic years of his professorship Wallis recognised and drew on the best ideas of the 1630s: Cavalieri’s indivisibles and the new possibilities of algebraic geometry opened up by Descartes. Not all of Wallis’s results were new: Harriot and Fermat had foreshadowed not a few of them, but without publishing, so that the *Arithmetica infinitorum* brought much material for the first time into the public domain. Where Wallis was most innovative, however, was in his methods, especially in his bold attempt to handle infinite processes and infinitesimal quantities. His lack of rigour was criticised at the time as it has been since, but for Wallis the end justified the means, and the flood of new results which followed at the hands of Brouncker, Mercator and above all Newton left Wallis in no doubt about the impact and value of his work.

Wallis was also revolutionary in another way, less often recognised: in his hands geometric interpretations were increasingly stripped away as he re-wrote his mathematics in the language in which he was most at home, that of arithmetic and, increasingly, of algebra. Such a fundamental shift of mathematical perception was bound to provoke a counter-reaction, and both Hobbes and Fermat protested. But when Fermat in 1657 complained about ‘this method with Algebraic notation’ he sounded already as a voice from the past: only eight years

later Newton read the *Arithmetica infinitorum* and took mathematics into a new and algebraic future.

## Chapter 8

### Catching Proteus: the collaborations of Wallis and Brouncker

#### Summary

William Brouncker (c.1620-1684) was the inaugural President of the Royal Society. He and Wallis collaborated closely during the 1650s on some original and unusual mathematics, but while Wallis acquired a lasting reputation, Brouncker's work is no longer well known. In this chapter I analyse the joint work of Wallis and Brouncker and attempt to separate their respective contributions and very different mathematical styles. I also offer a possible reconstruction of Brouncker's discovery of a continued fraction for  $4/\pi$ . Brouncker emerges as a skilled and intuitive mathematician whose reputation among his contemporaries was well deserved.

William, Viscount Brouncker (c.1620-1684) was once described by Sir Kenelm Digby as one of 'the greatest mathematicians of the age'.<sup>689</sup> He was chosen by Charles II as the inaugural President of the Royal Society, and held the post unopposed for fifteen years (1662-1677). Today his name is not generally familiar, and it is not easy to understand why, among so many eminent and gifted colleagues, Brouncker was selected for such a prestigious position. In *The Royal Society its origins and founders*, Joseph Scott and Sir Harold Hartley asked: "Why was he chosen as the first President of the Royal Society rather than John Wilkins, John Wallis, Robert Boyle or Sir Robert Moray?" and suggested that Brouncker's appointment was made as much in recognition of his personal and political qualities as his mathematical skills.<sup>690</sup> That he was accepted and esteemed for so long, however, by so many distinguished early Fellows for 'his great abilities in all Natural and especially

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<sup>689</sup> Kenelm Digby to Thomas White 8 May 1658, in Wallis 1658, no. 41.

<sup>690</sup> Scott and Hartley 1960, 146-157; 147, 150-151.

Mathematical knowledge'<sup>691</sup> suggests that history has underrated him as a mathematician.

At the time of Brouncker's inauguration only a few pages of his mathematics had appeared in print, all of them at the instigation of Wallis.<sup>692</sup> Brouncker's finest work was all done in association with Wallis during the 1650s:<sup>693</sup> in addition to questions of quadrature and rectification they also began to explore continued fractions and problems in number theory, topics previously unknown in England and which remained undeveloped until taken up by Euler and Lagrange in the next century. This chapter presents a new analysis of their joint work, and attempts to separate their respective contributions and mathematical styles.

In many ways the partnership between Brouncker and Wallis was a curious one, for they came from different social strata, and opposing ends of the political spectrum. Brouncker was born about 1620 (the exact date is unknown) and is usually said to have entered Oxford at the age of sixteen but there is no firm evidence that this was the case, and he told Aubrey that he was 'of no university'.<sup>694</sup> He was proficient in languages and mathematics and, according to

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<sup>691</sup> Scott and Hartley 1960, 151.

<sup>692</sup> Brouncker's continued fractions are in Wallis 1655b, Proposition 191. His method for  $Nx^2 + 1 = y^2$  is in Wallis 1658, letters 17 and 19. His rectification of the semicubical parabola is in Wallis 1659. All three were reprinted in Wallis's collected works, see Wallis 1695, 355-478; 469-470; Wallis 1693, 757-860; 797, 802-807; Wallis 1695, 489-569; 552-553 respectively. Brouncker's work was also reported in *A treatise of algebra*, 293, 317-318, 364-365.

<sup>693</sup> Brouncker did some work on the cycloidal pendulum from 1661 onwards in connection with ideas put forward by Huygens. It is described in Scott and Hartley 1960, 150, as 'largely uninspired'. The only other surviving work by Brouncker is a brief refutation of a paper by Thomas Hobbes, also written in 1661, and also sent to Huygens. The post-1661 papers are not discussed here but references may be found in the bibliography compiled by Whiteside in Scott and Hartley 1960, 157.

<sup>694</sup> Biographical information on Brouncker has been taken from Dubbey 1970; Lee 1885; Aubrey 1898, I, 128-129. Of modern writers only Smith D.E. 1923, 411, has suggested, following Aubrey, that Brouncker did not attend Oxford as a young man (the misconception that he did seems to have arisen from his later honorary award of 'Doctor of Physick'). Smith regarded

Aubrey, 'addicted' to the latter. His father, Sir William, was made viscount of Castle Lyons in Ireland in September 1645 but died only two months later, so that his son inherited his title at the age of 25. Brouncker spent the Civil War years in Oxford and in 1647 his intellectual prestige and loyalty to the King were rewarded with the degree of 'Doctor of Physick'. Brouncker had neither completed the statutory fourteen years of study for medicine, nor ever practised it afterwards, but it was not uncommon at the time for such honours to be awarded for services rendered.<sup>695</sup>

Wallis's university position too was a reward for his wartime activities, but for the opposing side: he had deciphered letters captured from the King whom Brouncker had so loyally served. Wallis and Brouncker must first have met after the end of the Civil War and their cooperation was an example of common mathematical and scientific interests overriding ideological differences, though it also has to be said that Wallis was never reluctant to promote his own interests and would have welcomed the social connection with Brouncker. Aubrey, who disliked Wallis intensely, certainly ascribed such motives to him when he described him as: 'Dr Wallis (a most ill-natured man, an egregious lyer and backbiter, a flatterer and fawner on my Lord Brouncker and his Miss, that my Lord may keepe up his reputation)'.<sup>696</sup> It is possible that Wallis coached Brouncker for a time, and they also shared an interest in music: Brouncker translated and published Descartes' *Musicae compendiae* with his own commentary in 1653; Wallis in his later years also wrote on music and edited and published Greek texts on harmony.<sup>697</sup> Brouncker was probably based more in London than Oxford after the war, so their meetings must have been occasional, and in the later 1650s their discussions were often carried on through letters

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Brouncker as first among the 'minor writers' of the period but his description of Brouncker's mathematics is not entirely accurate.

<sup>695</sup> Frank 1997, 508-509.

<sup>696</sup> Aubrey 1992, 160.

<sup>697</sup> Brouncker 1653; Wallis 1677; Wallis 1698a, b, c; Wallis 1699, 1-508.

which have left us a detailed but largely unexplored insight into their working relationship.

### Squaring the hyperbola (1655)

Brouncker published only one piece of work entirely under his own name, a method for the quadrature of a hyperbola. It was not printed until 1668 but Wallis first referred to it in the dedication (to Brouncker) of his *Adversus Meibomii* in 1657 and his reference suggested that the method had been devised in connection with his and Brouncker's work on the quadrature of the circle (in 1655).<sup>698</sup> The Danish mathematician Nicholas Mercator was the first to publish a solution for the hyperbola, in his *Logarithmotechnia* of 1668; Wallis wrote an account of it for the *Philosophical transactions* and, always concerned that English mathematicians should be given their due, encouraged Brouncker to produce his own much earlier work.<sup>699</sup>

Brouncker's short article gives some interesting insights into the skills and methods he had developed during the years when he worked most closely with Wallis. His method of quadrature, described in detail elsewhere,<sup>700</sup> used rectangles to cover an area under a hyperbola in what would now be regarded as a fractal pattern, and Brouncker also attempted to prove that the resulting infinite series was convergent (an attempt which has been described as 'more soundly based than any later seventeenth-century convergence investigation').<sup>701</sup> Of particular interest in the present context is Brouncker's notation, for he began by writing a general term of his sequence as:

$$\frac{1}{\underline{a} \times \underline{a-1} \times \underline{a-2}} = \frac{1}{a^3 - 3a^2 + 2a} \quad (1)$$

which for the purpose of comparison with subsequent terms he transformed to:

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<sup>698</sup> Wallis 1695, 229-290; 231-232.

<sup>699</sup> Mercator 1668; Wallis 1668; Brouncker 1668.

<sup>700</sup> Coolidge 1949, 136-146; 141-146; Stedall 2000c, 296-298.

<sup>701</sup> Whiteside 1961a, 264.

$$\frac{16a^3 - 48a^2 + 56a - 24}{16a^6 - 96a^5 + 232a^4 - 288a^3 + 184a^2 - 48a}$$

The notation confirms the close relationship between his work and Wallis's: compare the fractions in (1) with those in the *Arithmetica infinitorum* (the first such general sequence ever to appear in print):<sup>702</sup>

$$\begin{aligned} & \frac{a}{a+1}, \quad \frac{a}{a+1} \cdot \frac{2a}{2a+1}, \quad \frac{a}{a+1} \cdot \frac{2a}{2a+1} \cdot \frac{3a}{3a+1}, \dots \\ &= \frac{a}{a+1}, \quad \frac{2a^2}{2a^2+3a+1}, \quad \frac{6a^3}{6a^3+11a^2+6a+1}, \dots \end{aligned}$$

The full extent of discussions between Brouncker and Wallis on the text of the *Arithmetica infinitorum* can only be surmised and it may be that Wallis's sequence of generalised fractions was inserted at Brouncker's suggestion: they have something of the appearance of a later addition. Certainly by 1655, and possibly earlier, Brouncker was a skilled manipulator of the new notation, at ease with algebraic generalisation. The significance of such achievements should not be underestimated: only Harriot, in unpublished work half a century earlier, had mastered anything of the same kind previously.

### Squaring the circle (1655)

In 1651 Wallis had embarked on what was to be his longest and finest piece of mathematical invention, a quest for the quadrature of the circle. His methods, as set out in the *Arithmetica infinitorum*, have been discussed in thesis Chapter 7, and as we have seen, he devoted long and patient effort to finding a value for  $\pi$  ( $4/\pi$ ) by numerical interpolation. The pages that describe the final stages are some of Wallis's most engaging writing, and convey with a humility and openness rare for Wallis his sense of frustration as he approached the solution only to see it slip his grasp.<sup>703</sup>

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<sup>702</sup> *Arithmetica infinitorum*, Proposition 126. Note that although they used Descartes' index notation, both Brouncker and Wallis denoted an unknown number by Harriot's  $a$  rather than Descartes'  $x$ .

<sup>703</sup> *Arithmetica infinitorum*, Proposition 189, *Scholium*.

*Quamquam enim hanc spes non exigua visa est affulsisse, lubricus tamen quem prae manibus habemus Proteus tam hic quam superius non raro elapsus, spem fefellit.*

Although no small hope seemed to shine, what we have in hand is slippery, like Proteus, who in the same way, often escaped, and disappointed hope.

The Greek god Proteus was a sea deity who had the gift of knowing the future, but who often refused to give answers and if left unfettered escaped by assuming new shapes; it would be hard to find a better metaphor for the elusiveness, in Wallis's treatment, of the number we now know as  $\pi$ .<sup>704</sup> By persistent effort, however, Wallis eventually succeeded in pinning down his quarry, and in Proposition 191, the culminating proposition of his book, he found  $\square$  to be equivalent to the fraction which he wrote as:

$$\square = \frac{3 \times 3 \times 5 \times 5 \times 7 \times 7 \times etc}{2 \times 4 \times 4 \times 6 \times 6 \times 8 \times etc.}$$

At this point Wallis showed his work to Brouncker who, according to Wallis, thought it through for himself and suggested an entirely different form for the fraction, as Wallis reported in a new section headed *Idem aliter* ('The same thing done differently').<sup>705</sup>

$$\square = 1 + \frac{1}{2 + \frac{9}{2 + \frac{25}{2 + \frac{49}{2 + \frac{81}{2 + \dots}}}}}$$

Wallis described this new form as *fractio, quae denominatorem habeat continue fractum*, 'a fraction whose denominator is continually broken', the first

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<sup>704</sup> Homer, *Odyssey*, book IV, lines 509-520: 'But we, shouting, fell upon Proteus, and threw our hands around him; nor had the old man forgotten his wily art. First he became a lion with noble mane, and then a serpent, a panther, a wild boar, he became rushing water and a soaring-leaved tree. But we held him firmly with patient mind until at last the cunning old man wearied and asked: "Which god conspired with you to seize me against my will?"'

<sup>705</sup> *Arithmetica infinitorum*, Proposition 191, *Idem aliter*, Wallis 1695, 469-470.



description of what has come to be known as a continued fraction. There was also an unexplained but correct remark that the sequence

$$1, \quad 1 + \frac{1}{3}, \quad 1 + \frac{1}{2 + \frac{9}{5}}, \quad 1 + \frac{1}{2 + \frac{9}{2 + \frac{25}{7}}}, \dots \quad (2)$$

gave increasingly good approximations, alternately too large and too small.

These were astonishing results. Wallis had led his readers through every twist and turn of his thinking on his way to finding his own fraction for  $\pi$ , but this new form of Brouncker's came quite out of the blue. There was nothing in the *Arithmetica infinitorum*, or in any known English mathematics up to this point, to prepare the reader for it. Wallis clearly realised that some explanation was needed but failed to persuade Brouncker to 'show his working' and so attempted the task himself in a lengthy *Scholium*.<sup>706</sup> Unfortunately his description of Brouncker's method does not take us very far. He began by setting out Brouncker's starting points, the identities:

$1 \times 3 = 2^2 - 1$	$0 \times 2 = 1^2 - 1$
$3 \times 5 = 4^2 - 1$	$2 \times 4 = 3^2 - 1$
$5 \times 7 = 6^2 - 1$	$4 \times 6 = 5^2 - 1$
$\dots$	$\dots$

He continued:

*Quaerebat igitur qua ratione augendi erant factores, . .*

[Brouncker] asked, therefore, by what fraction the factors should be increased to give not these squares reduced by 1, but the squares themselves.

In other words, according to Wallis, Brouncker was looking for numbers which can be denoted by  $A, B, C, \dots$  a little larger than  $1, 3, 5, \dots$  with the property that:

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<sup>706</sup> *Arithmetica infinitorum*, Proposition 191, *Scholium to Idem aliter*, Wallis 1695, 470-476.

$$\begin{aligned}
AB &= 2^2 \\
BC &= 4^2 \\
CD &= 6^2 \\
&\dots \qquad (3)
\end{aligned}$$

(There is an analogous sequence for the odd squares but the present discussion will be restricted without loss of generality to the even squares.) This was all the help Wallis seemed able to give; from here he went straight to the final outcome without offering any further clue to the intermediate steps:

*Invenit autem id fieri posse, . .*

[Brouncker] found that this is possible, if each factor is increased by a fraction with the denominator infinitely broken, in the form we have shown above.

That is to say:

$$A = 1 + \frac{1}{2 + \frac{9}{2 + \frac{25}{2 + \dots}}}, \quad B = 3 + \frac{1}{6 + \frac{9}{6 + \frac{25}{6 + \dots}}}, \quad C = 5 + \frac{1}{10 + \frac{9}{10 + \frac{25}{10 + \dots}}}, \dots \quad (4)$$

Wallis had explained what Brouncker had set out to do, but not *why*; what he had achieved but not *how*. There was not even any proof that Brouncker's first fraction,  $A$ , was equivalent to  $\square$ .<sup>707</sup> All Wallis added further was the beginning of a proof that successive partial values of Brouncker's fractions multiplied together did in fact approximate more and more closely to squares. The notation ( $F_q$ ,  $F_c$ , etc.) was derived from Oughtred, and was typical of Wallis rather than Brouncker; the 'proof' seems to have been Wallis's attempt to provide some footing where he was uncomfortably out of his depth. His argument is quoted here to show how he handled such things:

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<sup>707</sup> Wallis always denoted Brouncker's first fraction by  $\square$ . He, or Brouncker, labelled the subsequent fractions  $B$ ,  $C$ ,  $D$ , . . as above, but used  $A$  for an altogether different purpose later on.

From any [pair] of the given fractions, the integer part of the first is  $F$  and of the next is  $F+2$ . The number between (to be squared) is  $F+1$ . Their product  $F_q+2F$  is less than its square  $F_q+2F+1$ .

Now add to each factor its first fractional part.

$F+\frac{1}{2F}$  multiplied by  $F+2+\frac{1}{2F+4}$  gives

$$\frac{4F_{qq} + 16F_c + 20F_q + 8F + 9}{4F_q + 8F} \quad \text{which exceeds the square}$$

$$F_q+2F+1 = \frac{4F_{qq} + 16F_c + 20F_q + 8F}{4F_q + 8F}.$$

Then add the second fractional part;

$F+\frac{1}{2F+\frac{9}{2F}}$  and  $F+2+\frac{1}{2F+4+\frac{9}{2F+4}}$  multiplied together are

$$\frac{16F_{cc} + 96F_{cq} + 280F_{qq} + 480F_c + 649F_q + 594F}{16F_{qq} + 64F_c + 136F_q + 144F + 225}$$

which is less than  $F_q+2F+1 =$

$$\frac{16F_{cc} + 96F_{cq} + 280F_{qq} + 480F_c + 649F_q + 594F + 225}{16F_{qq} + 64F_c + 136F_q + 144F + 225}.$$

At this point it is not surprising that Wallis felt he had gone far enough and he concluded with the comment *Et sic quousque procedatur* ('And so on as far as you like'). Wallis never returned to the problem of how made his discovery, and in *A treatise of algebra* thirty years later he merely re-stated the result.<sup>708</sup>

Succeeding generations of mathematicians, however, have continued to be intrigued by what Brouncker did. Euler, who was the first to develop a general theory of continued fractions, derived Brouncker's fraction easily from Leibniz' series:<sup>709</sup>

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<sup>708</sup> *A treatise of algebra*, 317-318.

<sup>709</sup> Euler 1748, paragraph 369.

$$\frac{\pi}{4} = 1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \dots$$

which has the same partial convergents as Brouncker's form of  $1/\square$  but no formal proof of the equivalence between Wallis's fraction and Brouncker's was given until 1872, by the German mathematician Bauer, using determinants.<sup>710</sup> In the twentieth century, work on Brouncker's fraction has been done by Brun, Hofmann, Whiteside and Dutka, all of whom have published derivations of Brouncker's fraction, but all using modern notation and concepts of functions which were eighty years or more into the future when Wallis and Brouncker were at work.<sup>711</sup> In the mid seventeenth century, as has already been noted, even the use of generalised algebraic expressions was still rare, and the examples in the *Arithmetica infinitorum* were among the first (Wallis's use of  $F + \frac{1}{2F}$  was one of them). Wallis and Brouncker were highly competent manipulators of the new algebra, as shown by the ability of both of them to handle large polynomial fractions, and their skill was based on a thorough understanding of the underlying arithmetic, but beyond this neither had any special techniques at their disposal, nor so far as is known, any previous work to serve as a model.

Given Wallis's few clues, any attempt to rediscover Brouncker's method inevitably involves a certain amount of guesswork, but it is instructive to make such an attempt within the confines of mid seventeenth-century notation and technique.<sup>712</sup> What follows is my own suggestion as to how Brouncker might have proceeded.

The first question to answer is *why* Brouncker set out to search for a decomposition of squares into number pairs in the first place. The answer

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<sup>710</sup> Bauer 1872.

<sup>711</sup> Brun 1951; Hofmann 1960; Whiteside 1961a, 210-213; Dutka 1981.

<sup>712</sup> The remarks in Scott and Hartley 1960, 149, that Brouncker 'had merely to find a particular function' and that the details are 'readily reconstructed' evade the issue and belie the true nature and originality of Brouncker's achievement. Whiteside has created but never published, his own reconstruction of Brouncker's work in the Latin, and even the typeface of the original, and I am grateful to him for sharing his insights into this rich piece of mathematics.

accepted or implied by most modern commentators is that Brouncker started from the repeating squares in Wallis's form of the fraction:

$$\square = \frac{3^2 \times 5^2 \times 7^2 \times \dots}{2 \times 4^2 \times 6^2 \times \dots}$$

That is to say, Brouncker may have argued somewhat as follows:

$$\begin{aligned} \square &= \frac{3 \times 3 \times 5 \times 5 \times 7 \times \dots}{2 \times 4 \times 4 \times 6 \times 6 \times \dots} \\ &= 2 \times \frac{1 \times (3 \times 3) \times (5 \times 5) \times \dots}{(2 \times 2) \times (4 \times 4) \times (6 \times 6) \times \dots} \\ &= \frac{(2 \times 2) \times (6 \times 6) \times (10 \times 10) \times \dots}{1 \times (4 \times 4) \times (8 \times 8) \times (12 \times 12) \times \dots} \\ &= \frac{AB \times CD \times EF \times \dots}{1 \times BC \times DE \times FG \times \dots} \quad (\text{assuming (3) is possible}) \\ &= A \end{aligned}$$

Such an approach would explain both why Brouncker needed a decomposition of squares and why Wallis took it for granted that  $\square$  and  $A$  were equivalent, and it serves as useful starting point for what follows. I will suggest later, however, that Brouncker had a different and much deeper motivation for his work.

As to *how* Brouncker found his decomposition, there seems no reason to doubt Wallis's assertion that he started from the identities:

$$\begin{aligned} 2^2 - 1 &= 1 \times 3 \\ 4^2 - 1 &= 3 \times 5 \\ 6^2 - 1 &= 5 \times 7 \\ &\dots \end{aligned} \quad (5)$$

Brouncker needed to increase the factors on the right hand side to produce exact squares on the left. A well known approximation to  $\sqrt{n^2 + 1}$  was  $n + \frac{1}{2n}$  so a useful starting point would have been to consider, as an upper bound, the pairs:

$$2^2 \approx \left(1 + \frac{1}{2}\right) \times \left(3 + \frac{1}{6}\right)$$

$$4^2 \approx \left(3 + \frac{1}{6}\right) \times \left(5 + \frac{1}{10}\right)$$

$$6^2 \approx \left(5 + \frac{1}{10}\right) \times \left(7 + \frac{1}{14}\right)$$

$$\dots \quad (6)$$

Each product here is too large,<sup>713</sup> but can be corrected by increasing the denominator of the first fraction:

$$2^2 = \left(1 + \frac{1}{2+a}\right) \times \left(3 + \frac{1}{6}\right)$$

$$4^2 = \left(3 + \frac{1}{6+b}\right) \times \left(5 + \frac{1}{10}\right)$$

$$6^2 = \left(5 + \frac{1}{10+c}\right) \times \left(7 + \frac{1}{14}\right)$$

$$\dots \quad (7)$$

These equations yield  $a = \frac{2}{5}$ ,  $b = \frac{2}{7}$ ,  $c = \frac{2}{9}$  and so on, that is:

$$2^2 = \left(1 + \frac{1}{2+\frac{2}{5}}\right) \times \left(3 + \frac{1}{6}\right)$$

$$4^2 = \left(3 + \frac{1}{6+\frac{2}{7}}\right) \times \left(5 + \frac{1}{10}\right)$$

$$6^2 = \left(5 + \frac{1}{10+\frac{2}{9}}\right) \times \left(7 + \frac{1}{14}\right)$$

$$\dots \quad (8)$$

Wallis's results required, however, that Brouncker had to maintain the underlying pattern of (3) and (6): the second factor of  $2^2$  had to be the same as the first factor of  $4^2$ , and so on. A revised approximation which kept this pattern would be:

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<sup>713</sup> The excess for each  $n^2$  is  $\frac{2}{4(n^2-1)}$ .

$$\begin{aligned}
2^2 &\approx \left(1 + \frac{1}{2 + \frac{9}{2}}\right) \times \left(3 + \frac{1}{6 + \frac{9}{6}}\right) \\
4^2 &\approx \left(3 + \frac{1}{6 + \frac{9}{6}}\right) \times \left(5 + \frac{1}{10 + \frac{9}{10}}\right) \\
6^2 &\approx \left(5 + \frac{1}{10 + \frac{9}{10}}\right) \times \left(7 + \frac{1}{14 + \frac{9}{14}}\right) \\
&\dots
\end{aligned} \tag{9}$$

These pairs are a better approximation than (6) but are now slightly too small; again a correct value can be obtained by increasing the final denominator of the first fraction:

$$\begin{aligned}
2^2 &= \left(1 + \frac{1}{2 + \frac{9}{2+a'}}\right) \times \left(3 + \frac{1}{6 + \frac{9}{6}}\right) \\
4^2 &= \left(3 + \frac{1}{6 + \frac{9}{6+b'}}\right) \times \left(5 + \frac{1}{10 + \frac{9}{10}}\right) \\
6^2 &= \left(5 + \frac{1}{10 + \frac{9}{10+c'}}\right) \times \left(7 + \frac{1}{14 + \frac{9}{14}}\right) \\
&\dots
\end{aligned} \tag{10}$$

yielding  $a' = \frac{25}{7}$ ,  $b' = \frac{25}{9}$ ,  $c' = \frac{25}{11}$ , and so on.

Manipulation beyond the level indicated here rapidly becomes extremely cumbersome, but by now Brouncker would have had his pattern and could, like Wallis, assume an *et sic quousque procedatur*. This approach explains both the natural emergence of continued fractions through the refined approximations at (7), (10), . . . and also the little noticed sequence of approximations alternately too

large and too small at (2): after 1 and  $1\frac{1}{3}$  each subsequent fraction is the first 'factor' of  $2^2$  in successive calculations (8), (10), . . .<sup>714</sup>

The apparent simplicity of the above procedure should not lead us to underestimate Brouncker's genius and intuition in devising this or something like it. It is no less remarkable that he regarded 'continually broken' fractions as an acceptable solution. Up to this time such fractions had appeared only twice in print: first in Bombelli's *L'algebra* of 1572, and then in Cataldi's *Trattato del modo brevissimo* in 1613.<sup>715</sup> Both books were published in Bologna and it seems very unlikely that Wallis or Brouncker in 1655 had seen either. Bombelli's *L'algebre* has never been part of the otherwise superb collection of late sixteenth-century mathematical texts in Oxford's Savile Library and Wallis gave it no serious attention until many years later.<sup>716</sup> He made several references to it in the Latin edition of his *Treatise of Algebra* in 1693 but hardly any in the original English edition in 1685, and never to continued fractions. Of some two dozen books and treatises that Cataldi published between 1572 and 1622 (all in Italian) not one is to be found even now in the Bodleian Library, suggesting that they never came into the hands of the seventeenth-century English collectors of

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<sup>714</sup> Further support for the plausibility of this reconstruction can be found in Wallis's description of its essential features, though in another context, much later in the same *Scholium*, Wallis 1695, 475: 'The first fraction increases the quantity and by so much that what was less becomes greater. If, keeping the same numerator, the denominator of this same fraction is increased (which may be done by addition of a second fraction), the first fraction and hence the total quantity are decreased. This decrease will be smaller (and the total quantity greater) the more the denominator of the second fraction is increased; which may be done by the addition of a third fraction. The third fraction decreases the second, and hence increases the first and also the total quantity. And similarly in what follows.'

<sup>715</sup> Bombelli 1572; Cataldi 1613. A facsimile of Cataldi 1613, 70, where his continued fractions first appear, can be found in Fowler 1987, 734-735, a good mathematical and historical introduction to continued fractions.

<sup>716</sup> Not only was Bombelli's book unknown to Wallis and Brouncker but it seems that Cataldi never saw it either, despite the fact that, like his, it was published in Bologna.



mathematical texts.<sup>717</sup> If this was so, Brouncker was reinventing continued fractions for himself.

Not only the form was new. The precise nature of Brouncker's fractions went well beyond anything comprehended by earlier mathematicians, for whom the various types of 'irrational' number defined by Euclid in *Elements* X, were demanding enough. Euclid's irrationals (and Cataldi's continued fractions) required no operation beyond the extraction of square roots, and were therefore what would now be termed 'algebraic'; Brouncker, on the other hand, had introduced at a stroke a whole sequence of 'transcendentals'. Clearly he and Wallis had a correct sense of how each fraction converged<sup>718</sup> and that for them was enough to justify their use. Wallis, however, had already foreseen that the numbers needed for the squaring of the circle must be of a different kind from anything previously known and had written as much only a few pages earlier.<sup>719</sup> Did Brouncker have the same deep understanding of what he had found?

Ironically, Brouncker's very success in substituting one value of  $\pi$  for another has done much to obscure the full subtlety of his mathematics. Later commentators, from Wallis in 1685 onwards, have focused only on Brouncker's fraction for  $\pi$  and in doing so have limited perceptions of his achievement to that alone. In other words, posterity has followed Wallis's agenda rather than Brouncker's. Not only has the sequence (2) of partial convergents been largely overlooked but so too has the existence and significance of the entire infinite

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<sup>717</sup> Several of Cataldi's treatises can now be found in the British Library but their relatively modern and uniform bindings suggest that they were acquired long after they were originally published.

<sup>718</sup> The fractions can be evaluated to any degree of accuracy except for  $0 + \frac{1}{0+} \frac{9}{0+} \frac{25}{0+} \dots$  which as it stands is meaningless, but to fit correctly in the sequence (as Wallis assumed it would) it must converge to a finite value beginning 0.455 . .

<sup>719</sup> See thesis Chapter 7.

sequence (4) of fractions A (or  $\square$ ), B, C, D, E, . . .<sup>720</sup> Yet as the above reconstruction or any similar attempt shows, it is impossible to arrive at Brouncker's fraction for  $\square$  without bringing the complete sequence in its train. Further, it was the sequence *as a whole* that Wallis and Brouncker needed. In Proposition 190 of the *Arithmetica infinitorum* Wallis had been forced to leave half-finished a generalisation of his interpolative process.<sup>721</sup> Essentially what he wanted there was to replace cumulative multipliers like  $\frac{3}{2} \times \frac{4}{3} \times \frac{5}{4}$  composed of an *odd* number of terms by equivalent multipliers with an *even* number of terms. Brouncker's fractions resolved the problem beautifully, for now  $\frac{3}{2}$ , for example, could be written as:

$$\frac{3}{2} = \frac{6}{4} = \frac{6 \times 6}{4 \times 6} = \frac{CD}{4 \times 6} = \frac{C}{4} \times \frac{D}{6}$$

The completion of Wallis's process occupies a large part of the *Scholium* to the *Idem aliter*.<sup>722</sup> This final generalisation has previously passed almost unnoticed because readers of Wallis, like commentators on Brouncker, have arrived at the fraction for  $\square$  and failed to look any further.<sup>723</sup> Brouncker, however, not only had a very clear understanding of what Wallis was trying to do but also probably played a large part in finishing it. Wallis ended the *Idem aliter* by saying:<sup>724</sup> '*Up to here I have set out his Lordship's thinking with what brevity and clarity I could*'. I would go so far as to suggest that the prime motivation for Brouncker's work was

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<sup>720</sup> None of the papers cited in note 711 makes any direct reference to the existence of fractions other than  $\square$ . Dutka 1981 comments on the partial convergents but refers in his title and throughout his paper to Brouncker's *fraction* in the singular only.

<sup>721</sup> Proposition 190 is not quite a 'proposition' in the sense now usually understood but, rather, indicates a fresh stage in Wallis's thinking.

<sup>722</sup> *Arithmetica infinitorum*, 184-191. Wallis 1695, 471-474.

<sup>723</sup> The generalisation of Proposition 190 is described in Whiteside 1961a, 241. Nunn 1910-11 and Scott 1938, 26-64, in otherwise detailed expositions of the *Arithmetica infinitorum* make no mention of either the Proposition or its completion.

<sup>724</sup> *Arithmetica infinitorum*, 194; Wallis 1695, 476, my italics.

not a search for an alternative form of  $\square$  but the completion of Wallis's Proposition 190. Wallis's own words again lend credence to this view:<sup>725</sup> '*Since I was showing him some of my progressions and the rule by which they proceeded, I asked him at the same time in what form he thought that quantity [ $\square$ ] might best be set out*'. In other words the new form of  $\square$  was incidental to a much deeper search on Wallis and Brouncker's part for a general completion of Wallis's interpolations. To credit Brouncker only with a single fraction is to miss the true significance of what he achieved.

Brouncker himself would have had a through understanding of the higher fractions in the sequence and almost certainly made use of them to calculate  $\pi$  as:

<sup>726</sup>

$$> 3.14159\ 26535\ 69^+$$

$$< 3.14159\ 26536\ 96^+$$

in agreement with the values found by Van Ceulen using the traditional method of inscribed and circumscribed polygons.<sup>727</sup> Brouncker's fraction for  $\square$  converges too slowly to have been useful here but the later fractions with their larger denominators converge much faster. Each is a rational multiple of  $\square$  or its reciprocal (as is easily seen from (3)) and their relative values were easily calculated. In modern notation:

$$\square = 4/\pi$$

$$B = \pi$$

$$C = 16/\pi$$

$$D = 9\pi/4$$

$$E = 256/9\pi$$

..

Values of  $\pi$  calculated from the first 33 fractions up to the seventh partial convergents, reveal no pairs corresponding exactly to those found by Brouncker.

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<sup>725</sup> *Arithmetica infinitorum*, 181; Wallis 1695, 469, my italics.

<sup>726</sup> Wallis to Digby 6 June 1657, Wallis 1658, no. 5.

<sup>727</sup> Van Ceulen 1619.

The closest fit comes from the 4<sup>th</sup> and 5<sup>th</sup> partial convergents of the fraction  $M = 25 + \frac{1}{50+..}$  which yield respectively:

3.14159 26536 40

3.14159 26535 88

close to, but slightly better than, Brouncker's values. In calculating to this degree of accuracy Brouncker must have discovered and used the method of evaluating continued fractions 'from the top down' by the recurrence relations set out (algebraically) at the end of the *Scholium*.<sup>728</sup> M is computationally one the most efficient fractions in the sense of balancing rapidity of convergence against the effort then needed to produce a value of  $\pi$ . Calculation by hand is also made easier by the repetition of 50 in each new denominator. Nevertheless, Brouncker must have gone through a great deal of trial and error and such calculation without mechanical aid is not for the faint-hearted: Proteus remains elusive to the end. Brouncker's calculation of  $\pi$  is another of his remarkable but unsung achievements.

### **Rectifying the semicubical parabola (1657)**

In 1657 William Neile in England and Hendrick van Heuraet in the Netherlands independently found ways of calculating the length of a curve which Wallis later named the 'semicubical parabola' (in modern notation  $y^2 = kx^3$ ). Neile's rectification was discussed by some of the members of the Gresham College meetings which later evolved into the Royal Society, and Brouncker and Wallis in particular paid close attention to it and reworked it for themselves in their own ways. Once again, it was Wallis who published the results. The three methods, Neile's, Brouncker's and his own, were written out in a long letter to Huygens which was appended to Wallis's *De cycloide*, allowing us to see clearly the contrasts between the three styles.<sup>729</sup>

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<sup>728</sup> *Arithmetica infinitorum*, 191-192; Wallis 1695, 474-475.

<sup>729</sup> Wallis to Huygens November 1659, Wallis 1695, 550-554. There is a brief description of Brouncker's method in Coolidge 1949, 139-141.

Neile's approach was essentially geometrical. He constructed, on the same axes, three curves, here denoted by A, B and C, whose equations in modern notation would be (A):  $y = x^{1/2}$ , (B):  $y = \frac{2}{3} kx^{3/2}$  and (C):  $y = (1 + k^2 x)^{1/2}$ . By dividing the areas so defined into infinitely thin strips, Neil could show that the length of (B) from  $x = 0$  to  $x = a$  was proportional to the area contained by curve (C) between the same ordinates; such an area was easily calculated since (C) was a parabola. There are two points to note here: first that Neile's method was based on geometrical ratios between lengths and areas, and second that he did not actually calculate any lengths for (B) but showed only that such calculations were in principle possible.

Brouncker followed Neile's method exactly, but turned it from geometry to algebra. Like Neile, Brouncker kept to the language of proportion; he found that the length of the curve (B) from (0,0) to (a,c) was given by the following rule:

$$a \text{ to 'length' is as } 27ac^2 \text{ to } (4a^2 + 9c^2) \times \sqrt{(4a^2 + 9c^2)} \text{ minus } 8a^3$$

Modernising the notation, this gives the absolute value of the length correctly as:

$$\text{length} = \frac{(4a^2 + 9c^2)^{3/2} - 8a^3}{27c^2}$$

Once again, Brouncker had demonstrated his skill in algebraic generalisation.

Wallis's method retained more of the geometrical flavour of Neile's original and he did not engage in algebraic manipulation to anything like the same extent as Brouncker. Where Wallis did use algebraic shorthand his letters (usually capitals) were tied to the physical properties of the curves, for example D for diameter, L for *latus rectum*, in contrast to Brouncker's more abstract use of  $a$ ,  $b$ ,  $c$ .<sup>730</sup> Brouncker was indebted to Neile for the underlying method, but the full transformation into algebra was very much his own.

### **The challenges from Fermat (1657-58)**

The publication of Wallis's *Arithmetica infinitorum* in 1655 gave rise indirectly to Brouncker's last major piece of mathematical invention. In 1656 the book came to

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<sup>730</sup> Wallis's abbreviations were those first introduced in his treatment of conics, Wallis 1655a.

the attention of Fermat in Toulouse and prompted him to write to ‘Wallis and other English mathematicians’ with some number problems that had engaged him for many years. It was not Wallis but Brouncker who initially took it upon himself to reply and who eventually did some of the best and most important work. Wallis became involved only at a later stage and for a short time worked intensively on Brouncker’s foundations, a reversal of their roles in the *Arithmetica infinitorum*. As before, however, it was Wallis who took the initiative in getting the work published, as the *Commercium epistolicum* in 1658, and contemporary and later mathematicians came to associate it with him as much as Brouncker.<sup>731</sup> The following account will attempt to distinguish more carefully between their respective contributions.

All the correspondence between England and France passed through Sir Kenelm Digby who had introduced Fermat to the newly published *Arithmetica infinitorum* in the summer of 1656. Digby, based in Paris, used the services of his friend and collaborator Thomas White for the journeys between England and France but the long delays (as much as two months) meant that events moved faster than the communication of them and it was not always clear to the participants themselves (nor to the modern reader) what had or had not been understood elsewhere. Wallis’s ordering of the letters in the published account does not always help to clarify the sequence of events. In addition the different dating systems in use in England and France occasionally make it appear that a reply was sent ahead of an original; for ease of reference in what follows, dates will be those used by the authors themselves.<sup>732</sup>

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<sup>731</sup> See for instance Weil 1983, 81, 92-97, 100 where Wallis and Brouncker are routinely mentioned in tandem and Brouncker’s method is described as ‘the one which Wallis credits to Brouncker.’ For a more careful distinction between the work of Wallis and Brouncker see Dickson 1919-23, II, 351-353.

<sup>732</sup> The letters will be printed in the forthcoming edition of Wallis’s correspondence edited by Scriba and Beeley who consistently use the Julian dating system (ten days behind the modern Gregorian calendar) for both English and continental letters.

Michael Mahoney has given an excellent account of Fermat's side of the story, and the reasons for his eagerness to engage with Wallis in what are now described as problems in number theory.<sup>733</sup> Fermat had devoted much of his life to such researches but had so far failed to interest any of his fellow French mathematicians. Pascal, as recently as 1654, had rejected Fermat's offerings with the words: 'I confess to you that they go right past me; I am capable only of admiring them and of begging you very humbly to take the first opportunity to complete them.'<sup>734</sup> On reading the *Arithmetica infinitorum* in 1656 Fermat must have supposed that Wallis might be more interested than Pascal in taking up his ideas but, ever reluctant to reveal just how much he knew, Fermat made his proposal in January 1657 in the form of a public challenge from Narbonne (southern) France to Celtic (northern) France, England and Holland. Fermat's target in northern France was Bernard Frenicle de Bessy, with whom he had corresponded for many years over such problems.<sup>735</sup> The version sent to England, although received first by Brouncker, was directed specifically to Wallis:<sup>736</sup>

A challenge from M. Fermat for D. Wallis, with the hearty commendations of the messenger, Thomas White.

The challenge itself was written in Latin:

*Proponatur (si placet) Wallisio et reliquis Angliae Mathematicis, sequens quaesitio numerica . .*

If it please them, let the following numerical problem be proposed to Wallis and the other English mathematicians.

First problem: To find a cube which, added to all its aliquot parts [divisors] makes a square. For example the number 343 is a cube of side 7. All its aliquot

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<sup>733</sup> Mahoney 1973, 332-347.

<sup>734</sup> Pascal to Fermat 27 Oct 1654, quoted in Mahoney 1973, 334.

<sup>735</sup> Mahoney 1973, 293-295, 337; Weil 1983, 51-91; Goldstein C. 1995, 21-33.

<sup>736</sup> Brouncker to Wallis 5 March 1657, *Commercium epistolicum*, no. 1.

parts are 1, 7, 49, which, added to 343, make the number 400, which is a square of side 20. Sought is another cube of the same kind.<sup>737</sup>

Second problem: Also sought is a square number which, added to all its aliquot parts, makes a cube number.

We await the solutions which, if England or Belgian and Celtic Gaul cannot give them, Narbonian Gaul will give and will offer and speak as a pledge of growing friendship to Mr Digby.

Brouncker received this in March 1657 and passed it on to Wallis who was dismissive.<sup>738</sup>

*Est autem ea questio eiusdem fere generis cum iis quae de numeris Perfectis . .*

The question is just about of the same sort as the problems ordinarily posed concerning the numbers called 'perfect', 'deficient' or abundant'. These problems, and others of the same sort, cannot at all or cannot completely be reduced to a general equation embracing all cases. Whatever the details of the matter, it finds me too absorbed by numerous occupations for me to be able to devote my attention to it immediately. But I can make at the moment this response: *the number 1 itself satisfies both demands*.<sup>739</sup>

To a mathematician of Fermat's standing, Wallis's response was an insult. The problems did indeed appear to be little more than simple number puzzles, but it

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<sup>737</sup> Fermat was not explicit about the kind of cube he had in mind, even whether or not it should be an integer. However, from his example, and his request for *alius cubus numerus ejusdem naturae* ('another cube number of the same kind') it may be assumed that he meant the cube of a prime (which would give rise to an equation of the form given at (11) in the text). The 'aliquot parts' of a prime cubed or squared are simply the lower powers of the same prime.

<sup>738</sup> Wallis to Brouncker 7 March 1657, *Commercium epistolicum*, no. 2.

<sup>739</sup> It is not at all clear what Wallis actually meant by this throwaway remark. In modern notation Fermat required integers  $p$  such that  $1 + p + p^2 + p^3$  is a square or  $1 + p + p^2$  is a cube. The first condition is satisfied by  $p = 1$  but the second is not. Wallis must have had in mind the trivial solution  $1 = 1^2$  (and  $1 = 1^3$ ). There is in fact only one solution to  $1 + p + p^2 + p^3 = q^2$  for prime  $p$ , the one given by Fermat ( $p = 7$ ). For proof see Mahoney 1973, 337-338 or Weil 1983, 87-91.



would seem that Fermat's real interest was in the equations which arise from a deeper exploration, equations of the form

$$Nx^2 \pm 1 = y^2 \quad (11)$$

with integer solutions.<sup>740</sup> Perhaps Fermat realised that he needed to be more explicit, for in February 1657 he issued a second challenge:<sup>741</sup>

*Dato quovis numero non-quadrato, dantur infiniti quadrati qui in datum numerum ducti, adscita unitate, conficiant quadratum. Exemplum . .*

Given any non-square number, there are given infinitely many squares which, multiplied by the given number and added to unity, make a square. Example: 3 is given, a non-square number; 3 multiplied by the square 1 and added to unity makes 4, which is a square. Again, the same 3, multiplied by the square 16 and added to unity, makes 49, which is a square. And, in place of 1 and 16, once can find infinitely many squares with the same property; we seek, however, the general canon, given any non-square numbers. What, for example, is the square which, multiplied by 149, or 109, or 433, etc. and added to unity, makes a square?

The second challenge was not even passed on to Wallis. Brouncker, however, answered both. Although the challenges had been posed in Latin, Brouncker replied in English, not expecting, he said later, that the letters would be passed on just as he wrote them.<sup>742</sup> For the same reason, Brouncker kept no copies of his early letters, but his solutions were repeated frequently in later correspondence. His response to the first challenge was hardly better than Wallis's; he suggested:<sup>743</sup>

$$\frac{343}{64} + \frac{1}{64} + \frac{7}{64} + \frac{49}{64} = \frac{400}{64} = \left(\frac{20}{8}\right)^2$$

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<sup>740</sup> The connection between Fermat's problems and equation (11) is not immediately obvious even to the modern reader. For an explanation see Mahoney 1973, 337-338.

<sup>741</sup> Brouncker to Wallis 11 September 1657, *Commercium epistolicum*, no. 8.

<sup>742</sup> Brouncker to Wallis 11 September 1657, *Commercium epistolicum*, no. 8.

<sup>743</sup> Wallis to Digby 27 September 1657, *Commercium epistolicum*, no. 9.

It was possible to construct infinitely many solutions of this type if one allowed that  $\frac{1}{64}, \frac{7}{64}, \dots$  were ‘aliquot parts’, or factors, of  $\frac{343}{64}$ , but this was a questionable assumption, and it was later contested bitterly by Frenicle. Fermat had not actually specified that the solutions should be integers: to him it was probably too obvious to need stating, though in fact this was the first time that problems requiring only integer solutions had been posed.<sup>744</sup> Brouncker’s solution to the second challenge was also in rational numbers. In modern notation, his solutions to the equation  $Nx^2 + 1 = y^2$  were:

$$x = \frac{2R}{N - R^2}; \quad y = \frac{N + R^2}{N - R^2} \quad (12)$$

based on the identity, for any  $R$ :

$$\frac{4R^2}{(N - R^2)^2} \cdot N + 1 = \frac{(N + R^2)^2}{(N - R^2)^2}$$

Compared with the results painstakingly obtained by Fermat, these offerings were desultory, but Brouncker, treading unfamiliar ground and unaware of Fermat’s work, must have felt that he had dealt with the problem satisfactorily. When Fermat received Brouncker’s response he found a young Englishman to help him translate it but the young man could not understand mathematics and Fermat could not tell whether Brouncker had really solved the problem or not. The fact that Brouncker appeared to have found it easy made Fermat suspect that he had not. It was certainly clear that Brouncker had failed to understand the need for integer solutions.<sup>745</sup>

Wallis, despite his expressed distaste for such problems, was drawn in by Fermat’s next move. In April 1657, quite independently of the number challenges, Fermat voiced his first criticisms of the *Arithmetica infinitorum*. He was inclined to damn his adversaries with very faint praise:<sup>746</sup>

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<sup>744</sup> Although the term ‘Diophantine’ is now often used to refer to problems with integral solutions, Diophantus (fl. 250AD) made no such restriction but sought rational solutions.

<sup>745</sup> Fermat to Digby 6 June 1657, *Commercium epistolicum*, no. 11.

<sup>746</sup> Fermat to Digby 20 April 1657, *Commercium epistolicum*, no. 4.

*J'ay leu l'Arithmetica Infinitorum de Wallisius, et j'en estime beaucoup l'auteur . .*

I have read the Arithmetica infinitorum of Wallis and I have great regard for its author. Even though the quadrature both of parabolas and infinite hyperbolas has been done by me many years since, and I formerly discussed it with the illustrious Torricelli, still I no less respect the inventions of Wallis, who no doubt did not know that I had pre-empted his work . .

As to what regards the quadrature of the circle in the said treatise, I am not fully persuaded of it . .

Wallis replied that Fermat's name had been completely unknown to him before his own work was published and that his quadrature of the circle was justified by Brouncker's calculation, given earlier. This was not enough to satisfy Fermat who went on to raise further arguments. As the correspondence progressed the number challenges became part of the wider exchange and Fermat's tone changed from condescension to outright provocation:<sup>747</sup>

*J'ose Vous dire avec respect et sans rien abbattre de la haute opinion, que j'ay de votre Nation . .*

I venture to say to you [Digby], with respect and without diminishing in the slightest the high opinion I have of your nation, that the two letters of Milord Brouncker, however obscure and badly translated, contain no solution at all. It is not that I mean thereby to renew the jousts and ancient tilting of lances which the English once carried out against the French. Rather, to continue the metaphor, I venture to maintain . . that accident and luck often intrude into scientific battles as much as in others, and that in any case we can say that 'no field can bear every crop.'

Fermat had a disingenuous knack of stirring up trouble with smooth sounding words. Despite his protestations to the contrary he had portrayed his challenges as a duel between the English and the French and was raising the stakes in a way the

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<sup>747</sup> Fermat to Digby 15 August 1657, *Commercium epistolicum*, no. 12.

English could not ignore. To keep the fires burning he went on to add another problem, clearly aimed at Wallis and Brouncker:

*Proponatur itaque, datum numerum cubum in duos cubos rationales dividere . .*

It is proposed to split a cube number into two cubes.

Similarly, to split a given number composed of two cubes into two other rational cubes.

We ask what England and Holland think of this matter?

As late as September 1657, before this letter arrived, Wallis re-iterated to Digby his opinion that Fermat's number problems had 'more in them of labour than either Use or Difficulty'.<sup>748</sup> He had still not even seen the second challenge, but knew that Brouncker had answered it, and continued: 'I know his Lordship so well, and his peculiar dexterity in things of that nature; that I have a very strong presumption of the accurateness of what he doth in such a way.' A few days later Brouncker sent him the second challenge and his own solution to it, and casually said that Wallis could, if he wished, send it to Digby in Latin to avoid further confusion.<sup>749</sup> Brouncker could, of course, have done so himself, but Wallis obliged: having so far contributed nothing to the work he now took up the correspondence and sent a formal letter to Digby summarising all the results so far.<sup>750</sup> From now on Wallis was to be the spokesman for the English side.

Fermat's rejection of Brouncker's efforts finally arrived in London early in October and was immediately sent on to Wallis in Oxford.<sup>751</sup> Brouncker realised that he had both misunderstood and trivialised Fermat's challenge and now

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<sup>748</sup> Wallis to Digby 3 September 1657, *Commercium epistolicum*, no. 7.

<sup>749</sup> Brouncker to Wallis 11 September 1657, *Commercium epistolicum*, no. 8.

<sup>750</sup> Wallis to Digby 27 September 1657, *Commercium epistolicum*, no. 9.

<sup>751</sup> Fermat to Digby 6 June 1657; Fermat to Digby 15 August 1657; *Remarques sur l'Arithmetique des Infinis*, undated but apparently written in August 1657, *Commercium epistolicum*, nos. 11, 12, 13.

worked intensively to find integer solutions to  $Nx^2 + 1 = y^2$ . In less than three weeks he was able to send Wallis a long list of solutions.<sup>752</sup>

For  $N = 2$  he found:

$$\begin{array}{ll} 2 \times 2^2 & (+ 1 = 3^2) \\ 2 \times 12^2 & (+ 1 = 17^2) \\ 2 \times 70^2 & (+ 1 = 99^2) \\ \dots & \end{array} \quad (13)$$

Brouncker noted that the sequence of 'x' values 2, 12, 70, . . . could be built up by regular multiplication from the first value, 2:

$$\begin{array}{l} 2 \times 5\frac{1}{1} = 12 \\ 12 \times 5\frac{5}{6} = 70 \\ 70 \times 5\frac{29}{35} = 408 \\ \dots \end{array}$$

and so, using 'Q' (quadratus) to indicate squaring, he compressed the left hand sides of equations (13) into the shorthand form:

$$2 \times Q: 2 \times 5\frac{1}{1} \times 5\frac{5}{6} \times 5\frac{29}{35} \times 5\frac{169}{204} \times \dots$$

It was in this concise form that he sent the solutions to Wallis with similar sequences for  $N = 3$ ,  $N = 5$  and others:

$$\begin{array}{l} 3 \times Q: 1 \times 3\frac{1}{1} \times 3\frac{3}{4} \times 3\frac{11}{15} \times 3\frac{41}{56} \times \dots \\ 5 \times Q: 4 \times 17\frac{1}{1} \times 17\frac{17}{18} \times 17\frac{305}{323} \times \dots \\ \dots \end{array}$$

Wallis had used such multiplicative sequences extensively in the *Arithmetica infinitorum* and would have understood them perfectly. But as usual Brouncker had been tantalisingly brief and had given no indication of how he had found his results. He gave only one clue as to a general procedure: putting  $R = a/e$  in his original solution (12) led to:

$$x = \frac{2ae}{(Ne^2 \sim a^2)}$$

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<sup>752</sup> Brouncker to Wallis 22 October 1657, *Commercium epistolicum*, no. 14.

(where  $Ne^2 \sim a^2 = |Ne^2 - a^2|$ ). So for the integer solutions required by Fermat the problem entailed finding pairs of numbers  $a, e$  such that  $(Ne^2 \sim a^2)$  divides  $2ae$ , or in modern notation:

$$(Ne^2 \sim a^2) \mid 2ae \quad (14)$$

(Note Brouncker's use here of the index notation  $a^2$ . Wallis in his September letter to Digby had retained the older form  $A_q$ . Fermat had used no notation at all but had set out all his challenges verbally.)

Once again Wallis took up the role of spokesman. On 21 November he wrote a long letter to Digby summarising the English achievements:<sup>753</sup>

*The first challenge:*  $1 + p + p^2 + p^3 = q^2$  or  $1 + p + p^2 = q^3$ . Here Wallis simply repeated his first observation that 1 (meaning, presumably,  $q = 1$  and  $p = 0$ ) satisfied both problems, and that Brouncker had also found a solution in fractions. Then, he complained petulantly, Fermat had demanded integer solutions only. The number 1, said Wallis, *was* an integer, and probably the number Fermat had thought of himself; there might indeed be others but Wallis did not think the problem worth the trouble of investigating further.

*The second challenge:*  $Nx^2 + 1 = y^2$ . Now Wallis, thanks to Brouncker, was on firmer ground. He repeated the solution already sent to Digby in September (still in the old notation: he wrote Fermat's equation as  $NF_q + 1 = L_q$ ). But now he also gave the result (14) found by Brouncker, and claimed triumphantly, giving several numerical examples, that 'we' (the English) could produce infinitely many solutions.

*The third challenge:* Partition of two cubes. Wallis noted that Frenicle had already solved this problem, and he added further solutions of his own, several of which were simply multiples of each other. He repeated his distaste for such problems, but had no doubt that Brouncker would be able to solve the companion problem of partitioning a single cube ( $z^3 = x^3 + y^3$ ). Such was his faith in Brouncker! Fermat knew that no such partition was possible.<sup>754</sup>

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<sup>753</sup> Wallis to Digby 21 November 1657, *Commercium epistolicum*, no. 16.

<sup>754</sup> Fermat 1891-1912, II, 431-436; 434, translated in Fauvel and Gray 1987, 365. This is the most easily proved case of what was to become known as 'Fermat's last theorem'.

On the same day that he set out this long letter for Fermat, Wallis also wrote to Brouncker advising him that it would be expedient to provide an explanation of his method as well as the results.<sup>755</sup> Wallis had been forced to make a similar request in connection with Brouncker's continued fractions two years earlier; this time Brouncker was more forthcoming and sent what is now the longest surviving example of his work. At the same time he evidently asked Wallis for suggestions as to how the method might be shortened.<sup>756</sup> Interested for the first time, Wallis at last gave the second challenge serious attention. His and Brouncker's differing approaches to the same problem demonstrate vividly their contrasting mathematical styles, and for this reason the outlines of each are given here in some detail.<sup>757</sup>

Wallis, using Brouncker's result (14), sought pairs of integers, (which he denoted by  $r$  and  $s$ ) such that  $Nr^2 \sim s^2 \mid 2rs$ . He tackled this problem with a will by working his way systematically and somewhat laboriously through the possible ways of writing  $Nr^2$  (or  $NQr$ ) in the form  $s^2 \sim 2rs$  (or  $Qs - 2rs$ ). Here is his first example, for  $N = 7$ :

$$7Q1 = Q3-2$$

$$7Q2 = Q6-8$$

$$7Q3 = Q9-18$$

$$7Q4 = Q12-32$$

$$7Q5 = Q15-50 \text{ \&c}$$

Wallis noted the pattern in the final non-square term,  $2rs$ , and continued:

$$7Q1 = Q3-2$$

$$7Q2 = Q6-8$$

$$7Q3 = Q9-18 = Q8-1$$

$$7Q4 = Q12-32 = Q11-9$$

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<sup>755</sup> Wallis to Brouncker 21 November 1657, *Commercium epistolicum*, no. 15.

<sup>756</sup> We have no record of this request which may have been made verbally, but it is clear in *Commercium epistolicum*, no. 17, that Wallis was responding to it.

<sup>757</sup> Both are to be found in Wallis to Brouncker 17 December 1657, *Commercium epistolicum*, no. 17. Wallis's method fills the main letter, Brouncker's method is appended.

$$7Q5 = Q15-50 = Q14-21$$

$$7Q6 = Q18-72 = Q17-37 = Q16 -4$$

Once again the final terms increased regularly, and from this Wallis was eventually able to predict where he would find pairs such that  $Nr^2 \sim s \mid 2rs$ . After a while, realising the need for generality he translated his results into algebra (this time using Cartesian notation), but his approach was essentially numerical, an exercise in pure pattern-spotting. By finding successive solutions for particular values of  $N$ , Wallis was eventually able to come up with a general result: given any non-square  $N$  and first solution integer  $r$ , let  $t = 2\sqrt{Nr^2 + 1}$ . Successive solutions are then given by:

$$\begin{aligned} &r.1 \\ &r.t \\ &r.(t^2 - 1) \\ &r.(t^3 - 2t) \\ &r.(t^4 - 3t^2 + 1) \\ &r.(t^5 - 4t^3 + 3t) \\ &\dots \end{aligned} \quad (15)$$

Setting  $N = 3$  and  $r = 1$ , for example, gave the sequence 1, 4, 15, 56, 209, . . . exactly as obtained by Brouncker. This work took up several pages to write out. One cannot but admire Wallis's dogged persistence and his willingness to engage in repeated calculations without, at first, seeing where they would lead: here if anywhere was the mind of a code-breaker at work.

Now compare this with Brouncker's brief explanation of *his* method, sent at Wallis's request. The method was general but Brouncker illustrated it with  $N = 13$ . Thus he needed  $13aa + 1$  to be a square and since  $9aa < 13aa < 16aa$  he could assume that the required solution was of the form  $3a + b$  (with  $b < a$ ). Hence:

$$13aa + 1 = 9aa + 6ab + bb$$

$$4aa + 1 = 6ab + bb$$

$a = 2b$  would make the left-hand side too large so now he had to have  $2b > a > b$ , and he put  $a = b + c$  (with  $c < b$ ). Hence (simplifying):



$$2bc + 4cc + 1 = 3bb$$

Now he needed  $2c > b > c$  so he put  $b = c + d$  and so:

$$3cc + 1 = 4cd + 3dd$$

Continuing in this way he eventually reached:

$$4hj + 3jj + 1 = 3hh$$

which has integer solutions  $h = 2j$ ,  $j = 1$ . Substituting back up the chain Brouncker arrived at  $b = 109$ ,  $a = 180$ , and the solution:

$$13. 180^2 + 1 = 649^2$$

Brouncker's process is essentially the Euclidean algorithm<sup>758</sup> for the h.c.f. of 649 and 180 (which guarantees that it is finite). It also leads to the continued fraction for  $\frac{649}{180}$  but it is impossible to say from the little Brouncker wrote whether he realised this.<sup>759</sup> Brouncker would almost certainly have known Euclid's algorithm but made no mention of the equivalence between Euclid's method and his own. Bachet had devised and published a similar technique for the solution of the Diophantine equation, in modern notation,  $Ax - By = 1$ ,<sup>760</sup> and Weil has suggested that Bachet's text served as a model for Brouncker; this may have been so but Brouncker's work was very much more sophisticated than that of Bachet who, like Euclid, was dealing only with linear forms.<sup>761</sup>

It is immediately clear that Brouncker's approach was strikingly different from Wallis's. Wallis began with repeated numerical calculations from which a general result eventually emerged; Brouncker's method was general and algebraic from the start. Wallis used algebraic notation only when he really needed it to express his results and was inconsistent in the symbols he used; Brouncker was

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<sup>758</sup> Euclid VII.1-3.

<sup>759</sup> If  $x$  and  $y$  are solutions of  $Nx^2 + 1 = y^2$  then  $\frac{y}{x} \approx \sqrt{N}$ . Brouncker's process actually defines the first period of the continued fraction for  $\sqrt{13}$ .

<sup>760</sup> Bachet 1612. Bachet gave his method in the second edition, 18-33. Although he used successive capital letters for each new number calculated, his method was described verbally. For details see Dickson 1919-23, II, 44-45.

<sup>761</sup> Weil 1983, 93.

fluent and masterly in his use of it. Wallis's approach was a perfect example of synthesis; Brouncker's of analysis.

Brouncker's method was acclaimed but never explored by his contemporaries beyond Wallis and Fermat.<sup>762</sup> Wallis noted the important repeating patterns which emerge if the process is continued far enough, suggested some short cuts and refinements and was able to find the general form (15).<sup>763</sup> Otherwise the method was to remain undeveloped until taken up by Euler, seventy years later. Ironically it was Euler who at the same time largely wrote Brouncker out of history. He read Wallis's account in the Latin translation of *A treatise of algebra* and wrote, as though Brouncker had never existed: 'Such problems have been agitated between Wallis and Fermat . . . and the Englishman Pell devised for them a peculiar method described in Wallis's works.'<sup>764</sup> Poor Brouncker! The equation  $Nx^2 \pm 1 = y^2$  has been mistakenly but universally known ever since as 'Pell's equation'.<sup>765</sup>

By the end of 1657 all the main results were in place, but the correspondence went on for some months more due to the late intervention of Frenicle (who had long ago arrived at his own solutions to the first challenge).<sup>766</sup> Frenicle knew nothing of Wallis and Brouncker's work until he first saw their earliest responses

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<sup>762</sup> Fermat never disclosed his method. Weil supposed that it was similar to Brouncker's, see Weil 1983, 93. For further discussion of Fermat's possible methods see Mahoney 1973, 328-332.

<sup>763</sup> Wallis to Brouncker 20 January 1658; Wallis to Brouncker 6 April 1658, *Commercium epistolicum*, nos. 19 and 29, and *A treatise of algebra*, Chapter 98. The repetitions noted by Wallis correspond to the (palindromic) periods in the continued fraction for  $\sqrt{N}$ .

<sup>764</sup> Euler to Christian Goldbach 10 August 1730, here quoted in the translation in Weil 1983, 174. Weil suggested that Euler read Brouncker's method in the *Commercium epistolicum* but if so it should have been more obvious that the method was Brouncker's. The Latin translation of *A treatise of algebra* precedes the *Commercium epistolicum* in Wallis 1699 where Euler would have read either. *A treatise of algebra*, in Chapters 57-60, makes many references to Pell, and in particular to his treatment of indeterminate Diophantine equations but not  $Nx^2 + 1 = y^2$ . The equation  $x = 12y^2 - z^2$  appears in Rahn-Pell 1668 but without any general treatment.

<sup>765</sup> For its full history see 'Pell equation', Chapter XII in Dickson 1919-23, II, 341-400. For a modern treatment in terms of quadratic forms see Weil 1983, 92-99.

<sup>766</sup> Frenicle 1657.

in February 1658 and wrote a scathing reply.<sup>767</sup> Wallis wrote back in early March with a long and pointless defence, but he knew by now that England had to show something better, and for the first time came up with non trivial solutions to the first challenge.<sup>768</sup> His approach was once again numerical not algebraic, but Frenicle had no interest in general solutions either and was suitably impressed. Finally, Frenicle challenged Wallis and Brouncker to prove the generality of their method by finding a solution for  $N = 313$ .<sup>769</sup> This was to be the acid test. Brouncker had no difficulty with it and sent his solution back to Digby in a brief and modest note on 13 March:<sup>770</sup>

Within the space of an hour or two at most this morning I found that  $313 \times Q7170685 - 1 = Q126862368$  and therefore that  $313 \times Q(2 \times 7170685 \times 126862368) = 1819380158564160 + 1 = Q32188120829134849$ . Which I thought fit to present you, because Mouns. Frenicle may thence perceive, that nothing is wanting in the perfect solution of that Probleme.

Neither brevity nor modesty were Wallis's style, and he followed up Brouncker's note with a much longer letter in Latin, confirming that Brouncker had indeed found a solution and that Frenicle should remain in no doubt that 'we' understood the method perfectly.<sup>771</sup>

These letters from Wallis and Brouncker in March 1658 with their numerical solutions set the seal upon their success. Frenicle conceded their victory; Digby, responded with a very long and effusive letter to Wallis.<sup>772</sup>

And I doubt not but that your last Letters of the 4 and 15 of March will make [Fermat and Frenicle] and all the world give as large and as full a deference to you. For although I had time, since receiving them, but to run them greedily

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<sup>767</sup> Frenicle to Digby 3 February 1658; Digby to Wallis 6 February 1658, *Commercium epistolicum*, nos. 22, 21.

<sup>768</sup> Wallis to Digby 4 March 1658, *Commercium epistolicum*, no. 23.

<sup>769</sup> Frenicle to Digby between 6-10 February 1658, *Commercium epistolicum*, no. 26.

<sup>770</sup> Brouncker to Digby 13 March 1658, *Commercium epistolicum*, no. 27.

<sup>771</sup> Wallis to Digby 15 March 1658, *Commercium epistolicum*, no. 28.

<sup>772</sup> Digby to Wallis 4 May 1658, *Commercium epistolicum*, no. 36.

over, yet I see enough of the redundant light in them to reverence, not a rising, but a noon day Sun in its very vertical point and highest Zenith.

Digby also wrote warmly to Brouncker but, exhausted by his praise of Wallis, very much more moderately and more briefly:<sup>773</sup>

I give you most humble and hearty thanks for yours, which I embrace with exceeding gladness, joy and respect . . Now, neither [Frenicle] nor Mouns. Fermat, will have any more to cavil at, either your Lordship, or Dr. Wallis; unto whom I have written at large (considering my inability of writing much at present) and do presume to beg your favour in conveying my Letter to him; which I leave open, that if you please you may cast your eye over it. . If I were not quite wearied out (I am yet so weak) with writing my Letter to Dr Wallis, your Lordship should not thus easily be delivered of my troubling you at this time; which for my mentioned reason I must not now further enlarge, but humbly kissing your hand, I rest,

My Lord

Your most humble and worthy servant

Kenelm Digby

Digby also wrote to Thomas White:<sup>774</sup>

Truly these last letters from his Lordship and the Doctor have wrought a mighty change in men's opinions of them. They are now looked upon as the greatest mathematicians of the age.

Fermat too acknowledged their success:<sup>775</sup>

*Illustrissimos Viros Vicecomitem Brouncker et Johannem Wallisium quaestionum numericarum a me propositarum solutiones tandem dedisse legitimas libens agnosco, imo et gaudeo . .*

I recognise, indeed I rejoice, that the illustrious gentlemen Viscount Brouncker and John Wallis have at last given legitimate solutions to the numerical

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<sup>773</sup> Digby to Brouncker 4 May 1658, *Commercium epistolicum*, no. 35.

<sup>774</sup> Digby to White 8 May 1658, *Commercium epistolicum*, no. 41.

<sup>775</sup> Fermat to Digby 19 June 1658, *Commercium epistolicum*, no. 46.

problem I set forth. The most noble gentlemen did not wish even for a single moment to confess themselves unequal to the proposed problems.

This last remark was another of Fermat's barbed understatements: it had taken Wallis many months to make any sort of serious response. Fermat went on to hope that the English mathematicians would maintain their new found skills:

The French will say that the English satisfied the proposed problems. But let the English say in turn that the problems were worthy of being proposed to them and let them not disdain in the future to examine and investigate more closely the nature of integers and also to foster this subject, in which they are esteemed for their subtlety and strength of mind.

Fermat's plea fell on deaf ears. Neither Wallis nor Brouncker ever did any further work on number problems.

The idea of publishing the correspondence must have been considered by Wallis as early as February 1658, for Van Schooten wrote a long letter in March, at Wallis's request, outlining the various Dutch responses for inclusion.<sup>776</sup> Many of Wallis's own letters, formal, lengthy and in Latin, have the appearance of having been written with publication in mind from the start. By contrast, those of Brouncker, if they survived at all, were little more than brief informal notes, almost always in English. Wallis took on a self-appointed role as chief correspondent but without Brouncker he might have had nothing to report. Not only was Brouncker the first to take up Fermat's challenges, several months ahead of Wallis, but it was he who first found a general and algebraic procedure for solving  $Nx^2 + 1 = y^2$  and laid a foundation on which future number theorists were to build.

### **Brouncker's contribution to mathematics**

With such fine work to his credit, it might have been hoped that Brouncker would go on to even greater things, but it was not to be. He left no mathematics of any great value from the period of his Presidency. The last significant reference to his

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<sup>776</sup> Van Schooten to Wallis 18 March 1658, *Commercium epistolicum*, no. 33.

mathematical prowess is in a letter from Collins to James Gregory in February 1669:<sup>777</sup>

. . the Square Roote here resembles somewhat of Division there supposing the Divisor equall to the quote and the Lord Brouncker asserts he can turne the square roote into an infinite Series . .

Collins' remark was based on the identity

$$\frac{(1-x^2)}{(1-x^2)^{1/2}} = (1-x^2)^{1/2}$$

in which the divisor is the same as the quotient. Assuming that  $(1-x^2)^{1/2}$  can be expressed as an infinite series  $a + bx + cx^2 + dx^3 + \dots$  it is a simple matter to evaluate the coefficients as  $b = d = f = h = \dots = 0$  and  $a = 1$ ,  $c = -\frac{1}{2}$ ,  $e = -\frac{1}{8}$ ,  $g = -\frac{1}{16}$  and so on. (Ironically, if this result had been available to Wallis in 1652 he would not have needed to resort to interpolation and we might not then have had either his fraction or Brouncker's.)

There are a number of reasons why Brouncker's reputation has faded over time. For Brouncker mathematics was simply a pleasing diversion, so that although he responded with great skill and originality to the problems posed by Wallis and Fermat he never needed, like Wallis, nor chose, like Fermat, to range over a wider field. In later years he became busy with other matters: in the years immediately after the Restoration in 1660 he became a Member of Parliament, the first President of the Royal Society, President of Gresham College and Commissioner for the Navy, so that it was probably only during a relatively short period in the 1650s that he had enough leisure to indulge his mathematical interests.

For Wallis, on the other hand, mathematics was a lifelong career which kept his name and his work in the public domain. Wallis also had a far keener sense than Brouncker of the value of claiming and publishing results, and without Wallis to provide the means of publication or the encouragement it is doubtful that we would have any of Brouncker's work at all. In the quarrel with Fermat,

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<sup>777</sup> Turnbull 1939, 66.

Wallis, as chief correspondent, came to be seen by contemporary mathematicians as the leading partner in the affair. This certainly seems to have been Digby's view, and it was his version of events that spread amongst the French mathematicians. The final adulatory letter from Frenicle mentioned Wallis by name no fewer than eleven times but Brouncker only once.<sup>778</sup> Van Schooten's letter to Wallis with the Dutch responses made no mention of Brouncker at all. Later historians have tended to bracket Wallis and Brouncker together without exploring their separate roles.

Wallis's work eventually filled three large volumes while Brouncker's entire published output amounts to less than a dozen pages. In its own time Wallis's work was undoubtedly the more influential: the *Arithmetica infinitorum* set the stage for some of the great English mathematics of the second half of the century while the topics explored by Brouncker were to some extent always outside the mainstream. It was not until a full century later that the deeper implications of Brouncker's work began to emerge. Euler, from 1759, recognised the close relationship between continued fractions and 'Pell's equation' by observing that Brouncker's algorithm for the latter produced the continued fraction expansion of  $\sqrt{N}$ .<sup>779</sup> Lagrange, following Euler, wrote three papers between 1768 and 1779 on continued fractions, the first of which included a definitive treatment of 'Pell's equation'.<sup>780</sup> If Brouncker's work lapsed into obscurity in the later seventeenth century it was perhaps because his successors, in England at least, were not yet ready for it.<sup>781</sup>

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<sup>778</sup> Frenicle to Digby 8 May 1658, *Commercium epistolicum*, no. 43.

<sup>779</sup> Euler 1765.

<sup>780</sup> For accounts of the work of both Euler and Lagrange and full references see Smith H.J.S. 1894, I, 193-195; Dickson 1919-23, II, xii, 354-364; Weil 1983, 229-232, 314-316.

<sup>781</sup> Huygens in 'Descriptio automati planetarii' (1691) used continued fractions calculated by the Euclidean algorithm to find appropriate gear ratios for his planetary models. He had first written down such fractions in 1680, see Huygens 1888-1950, XX, 389-394 and XXI, 628-643. Huygens commented on the ordinary fraction approximations for  $\pi$  in Chapters 10, 11 of *A treatise of algebra*, but made no mention of the continued fraction devised by Brouncker.

In his own day, Brouncker's genius, if not fully understood was clearly recognised, most of all by Wallis, his closest mathematical colleague. The mathematical styles of the two men were completely different yet complementary: Wallis persevering and systematic, occasionally a little dull; Brouncker original, intuitive and sure-footed. Wallis's approach to the problems they worked on together was primarily numerical, while Brouncker slipped easily into algebraic notation, and was able to present an entire argument in generalised form, a striking achievement for its time. Wallis was a mathematician who worked 'with much labour, and by many circuits and operations'.<sup>782</sup> Brouncker, on the other hand seemed to begin with a clear view of what he wanted and how to get there. Wallis was the synthesist; Brouncker the analyst. Wallis managed, in the end, to catch Proteus by circling in more and more closely, carefully covering every inch of ground; Brouncker simply took Proteus by the hand and kept his grip, unbemused by new or changing forms. For those who delight in original and unusual mathematics Brouncker's work still reveals a remarkable depth and richness, and without it seventeenth-century mathematics would shine a little less brightly than it does.

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<sup>782</sup> Actually Digby's description of Fermat, Roberval and Descartes, in Digby to Wallis 10 February 1658, *Commercium epistolicum*, no. 24.



## Chapter 9

### Conclusion: ‘many pretty things worth looking into’

This final chapter looks at reactions to *A treatise of algebra* immediately after its publication and since, and makes some concluding remarks about Wallis’s perspectives on algebra and on history.

#### Reactions to *A treatise of algebra*

A notice of *A treatise of algebra* appeared in the *Philosophical transactions* in July 1685,<sup>783</sup> but was not a review in the modern sense, simply a reprint of Wallis’s *Preface to the Reader* in which he had outlined the contents of the book. (The *Preface* was in turn almost identical to Wallis’s 1683 *Proposal*). A rather more interesting review written by Leibniz appeared the following year in the *Acta eruditorum*.<sup>784</sup> Leibniz, like Wallis, summarised for his readers the contents of *A treatise of algebra* but also added some telling points of his own. First, he had read the early Italian algebraists more carefully than Wallis had, and so was able to add to Wallis’s meagre list of sixteenth-century English mathematicians the name of Richard Wentworth who had once been a pupil of Tartaglia. Second, Leibniz made some small efforts to redress Wallis’s English bias: on the subject of rectification he named Van Heuraet before either Wren or Neile, and he considered Mercator (whom he regarded as German) to be prior to Newton in his discovery of the infinite series for  $\ln(1+x)$ . Third, Leibniz recounted Wallis’s claims for Harriot at some length and stated that Wallis and Pell took such claims seriously. Leibniz had met Pell personally when he visited London in 1673,<sup>785</sup> just about the time Wallis was beginning to write *A treatise of algebra*, and his remark is a further indication that Pell and Wallis were probably discussing Harriot at that time. Leibniz himself seemed to respect Wallis’s views on Harriot, perhaps the only continental mathematician who ever did so.

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<sup>783</sup> Wallis 1685b.

<sup>784</sup> Leibniz 1686.

<sup>785</sup> Journal book of the Royal Society for 24 April 1712, cited in Manuel 1980, 322.

We have little direct evidence of how the book was received by contemporary English readers except in a letter from Roger Cotes, then a young Cambridge student, to his uncle, John Smith, in 1698.<sup>786</sup> Cotes laid most emphasis on the material in the later chapters, of relevance to the development of the calculus:

I have Dr Wallis's Algebra I think I bought it very cheape I am very well pleased w<sup>th</sup> y<sup>e</sup> Book. The D<sup>r</sup>'s Buisness therein is to shew y<sup>e</sup> Original, Progress & Advancement of Algebra from time to time, and by what steps it hath attained to y<sup>t</sup> height at which it now is he give[s] a full Account of y<sup>e</sup> Methods used by Vieta Harriot Oughtred De-Chartes and Pell & others and of y<sup>e</sup> several methods of exhaustions, Indivisibles, Infinites, Approximations &c. amongst other things he speak's of squaring Curves and after other ways of approximations shewed he show's you this of Mr Newton he determin's it impossible to do y<sup>e</sup> business exactly. In my mind there are many pretty things in y<sup>t</sup> book worth looking into.

A continental mathematician who read the book with care in its English edition was Christian Huygens, who made notes headed 'Du livre de Wallis, Historia algebrae angliae' as part of his writing on the three classical problems of antiquity.<sup>787</sup> In connection with the quadrature of the circle he noted four chapters of *A treatise of algebra* in this order: Chapter 83, where Wallis ventured that the quadrature could not be done using numbers so far known; Chapter 79 where Wallis gave a detailed justification of the method of induction (on which Huygens had expressed serious doubt in 1656 when it first appeared in the *Arithmetica infinitorum*); Chapter 95 where Huygens' own quadrature of the circle was mentioned in the context of Newton's new method by infinite series; and Chapter 10 with its fractional approximations for  $\pi$ . This last was of particular interest to Huygens since he himself had found similar approximations in 1680 by a rather shorter method (he called Wallis's method 'bien longue'). He gave his notes the subheading 'Développement du *Numerus impossibilis* en une fraction continue' but this was to read into Chapter 10 more than Wallis had put there, for although the

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<sup>786</sup> Edleston 1850, 191.

<sup>787</sup> Huygens 1888-1950, XX, 367-403, 389-394.

work would have led naturally to a continued fraction expansion for  $\pi$ , Wallis himself had not gone so far. Brouncker's later discovery of continued fractions was never mentioned by Huygens.

In general Wallis's history drew more attention than his mathematics. That there were a number of criticisms can be inferred from revisions made when *A treatise of algebra* was translated into Latin in 1693 (see Appendix IV). There Wallis paid more attention to Bombelli, justified his lengthy treatment of Oughtred, and praised Kersey and one or two other English mathematicians he had previously neglected. The most important changes, however, were in his discussion of Harriot and Descartes. Morland had written to Wallis asking him to detail his accusations against Descartes,<sup>788</sup> but others gave Wallis no such chance to explain himself and took their derision straight into print. When Jean Prestet brought out the second edition of his *Elemens des mathematiques* in 1689 he wrote:<sup>789</sup>

*Ce ne'est que sur de vaines conjectures . .*

It is only on vain conjectures or from envy that some have wanted to make believe that [Descartes] took his method from others, and particularly from a certain English Harriot, whom he had never read, as he declared in one of his letters. And while Monsieur Wallis, a little too jealous of the glory with which France has acquitted herself in mathematics, has just renewed this ridiculous accusation, one is right not to believe it at all, for he speaks without proof.

When Baillet brought out his life of Descartes two years later he said that the story of Descartes' plagiarism, recently renewed by Wallis, had first been put about by Roberval but that Pell, Aylesbury and Warner had long ago discounted it.<sup>790</sup> It was this comment of Baillet's that finally provoked Wallis

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<sup>788</sup> Wallis to Morland 8 January 1689; Morland to Wallis 12 March 1689, Wallis 1693, 206-213.

<sup>789</sup> Prestet 1689, II, Preface (unpaginated), translation JS.

<sup>790</sup> Baillet 1691, Book VIII, 541. This material is not in the abridged English translation Baillet 1693. It was Pell who told the following story about Roberval and Sir Charles Cavendish which appears in *A treatise of algebra*, 198: 'I admire (saith M. Roberval) that notion in Des Cartes of putting over the whole equation to one side, making it equal to

into writing *De Harrioto addenda* and revealing Pell's support for all he had written.

Euler's reading of *A treatise of algebra* in the early eighteenth century and the inspiration he took from the mathematics of Brouncker were discussed in thesis Chapter 8. Most later eighteenth-century readers, however, like Wallis's contemporaries, responded to the history rather than the mathematics. English readers, not surprisingly, were better disposed towards the book in this respect than their continental counterparts. Nicholas Saunderson in 1749 recommended it without criticism to those of his Cambridge students who were interested in such things,<sup>791</sup> but Montucla was scathing, and all too often correct, about Wallis's poor treatment of Cardano, Bombelli, Viète and Descartes.<sup>792</sup> Hutton in 1796 was kinder, though one purpose of his own history was to counteract the 'superficial and partial' investigations of his predecessors, including Wallis.<sup>793</sup>

Even in the nineteenth century English mathematicians refrained from too strident criticism: de Morgan in 1838 recognised that the book had some shortcomings but still considered it 'full of interest';<sup>794</sup> Cayley, writing in the *Encyclopaedia Britannica* fifty years later, briefly referred to the views of Montucla and de Morgan but added no further comment;<sup>795</sup> Ball merely said

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Nothing, and how he lighted upon it. The reason why you admire it (saith Sir Charles) is because you are a French-man; for if you were an English-man, you would not admire it. Why so? (saith M. Roberval) Because (saith Sir Charles) we in England know where he had it; namely from Harriot's algebra. What Book is that? (saith M. Roberval,) I never saw it. Next time you come to my Chamber (saith Sir Charles) I will shew it you. Which after a while, he did: And upon perusal of it, M. Roberval exclaimed with admiration (Il l'a veu! Il l'a veu!) He had seen it! He had seen it! Finding all that in Harriot which he had before admired in Des Cartes; and not doubting but that Des Cartes had it from thence.' Pell's retelling of the story to Wallis does not lend credence to Baillet's view that he discounted Descartes' plagiarism.

<sup>791</sup> Saunderson 1740, 49.

<sup>792</sup> Montucla 1799-1802, I, III.3 and II, IV.6.

<sup>793</sup> Hutton 1796, vi.

<sup>794</sup> De Morgan 1838, 42.

<sup>795</sup> Cayley 1888, 331-332.

that Wallis's account contained 'a great deal of valuable information'.<sup>796</sup> Cantor, on the other hand, dismissed the histories of both Montucla and Wallis as 'inspired by excessive national pride'.<sup>797</sup>

Late nineteenth- and early twentieth-century historians generally disengaged themselves from the history in *A treatise of algebra* and concentrated on its mathematical content, but with some curious distortions of vision. Ball thought the book 'noteworthy as containing the first systematic use of formulae'<sup>798</sup> and cited as an example ' $v = st$ ', a formula nowhere to be found in it. The point which most interested Cajori in 1894 was that 'Wallis discusses the possibility of a fourth dimension',<sup>799</sup> but this too was a theme that the book barely touched on. Cajori also noted the attempts to give geometrical interpretations of complex numbers but said that Wallis 'failed to discover a general and consistent representation'. This was not true, and what Cajori probably meant was that Wallis had not come up with the standard modern representation. Scott's long discussion of *A treatise of algebra* in his biography of Wallis was almost entirely taken up with the account of Harriot, so much so that Scott claimed that Wallis's 'best work consists of filling in the gaps which Harriot had left'<sup>800</sup> and on the remaining seventy-five chapters of the book he was virtually silent. Perhaps the worst description of *A treatise of algebra*, however, is to be found in the 1974 *Encyclopaedia Britannica* article on Wallis (now on CD) which says:

Wallis published, in 1685, his *Treatise on Algebra* [sic], an important study of equations that he applied to the properties of conoids, which are shaped almost like a cone. Moreover in this work he anticipated the concept of complex numbers.

The first sentence possibly refers to results in the *Arithmetica infinitorum*. The second sentence is simply untrue: the concept of complex numbers was

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<sup>796</sup> Ball 1888, 292.

<sup>797</sup> Cantor 1894-1908, III, 4.

<sup>798</sup> Ball 1888, 292-293.

<sup>799</sup> Cajori 1894, 184.

<sup>800</sup> Scott 1938, 133-165;156.

already well established by Bombelli and Harriot, though Wallis did anticipate the modern representation of such numbers.

In recent years Whiteside and Fowler have investigated some of the deeper mathematical content in *A treatise of algebra* (see thesis Chapters 8 and 6). There is also a new awareness of its value, as of other early mathematical texts, in mathematics education. This thesis is therefore the latest, but almost certainly not the last, word on Wallis's 'large discourse concerning algebra'.

### Wallis's perspective on algebra

One of the most remarkable features of *A treatise of algebra* is how little explicit discussion it contains about what algebra was, or how it changed in the course of its evolution. To the modern reader these are unaccountable omissions in a book which purports to be a history, but Wallis probably assumed that algebra was familiar enough to need no definition; further, by retracing the steps by which algebra 'hath attained the Heighth at which now it is',<sup>801</sup> the reader would come to understand for himself how the subject had developed and improved. For a modern audience, however, it is perhaps helpful to draw out some of the implicit assumptions about what algebra was, and came to be.

In the opening pages of *A treatise of algebra* Wallis gave four labels by which algebra was commonly known: *analysis*, *al-jabr*, *regula cosae* and *ars magna*. The only one he expanded upon was *analysis*, but in doing so he shifted its meaning in an interesting way. Viète had seen analysis as a conceptual tool for the rediscovery of Classical theorems; Wallis, however, discussed analysis as a property of arithmetic, in which subtraction, division and extraction of roots were the analytic counterparts (we should now say inverses) of the synthetic operations of addition, multiplication and composition of powers. This tells us much about Wallis whose frame of reference was entirely different from Viète's and who saw algebra as primarily a generalisation of arithmetic (recall his description of Viète's algebra as 'specious arithmetick'). Wallis's description of *analysis* in fact brought his

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<sup>801</sup> *Treatise of algebra*, title page.

concept of algebra closer to the traditional *al-jabr* with its emphasis on manipulating equations by means of inverse operations.

Wallis's discussion of *analysis* also points to another unspoken assumption at the start of his treatment, that algebra was essentially about finding unknown quantities through the solving of equations. Only once did Wallis come close to making such a definition explicit, at the beginning of his section on Harriot's algebra. There he spoke of the 'nature of equations (*wherein lyes the main Mystery of Algebra*)'.<sup>802</sup> Recent historians of algebra have largely restricted themselves to a similar definition,<sup>803</sup> yet Wallis's treatment depicts a far richer landscape of algebraic activity. The nature of equations was in fact far from being the main theme of his book: the subject featured most prominently in connection with the work of Oughtred and Harriot and to a lesser extent of Pell, but in the final third of the book was barely mentioned. Wallis's treatment therefore reflected the trend of actual historical development, in which the study of equations was initially a powerful motivating impulse (as it was to be again later) but was overshadowed in the later seventeenth century by the richness of progress in other directions.

From the discussion in this thesis it is clear that a comprehensive definition of algebra has at least to include: describing and solving quadratic equations (al-Khwārizmī c.830); transforming equations by change of root (Cardano 1545); transcribing Euclid II into literal notation (Harriot c.1600); finding a general formula for figurate numbers (Harriot c.1600); describing an infinite recurring pattern by means of subscript notation (Brouncker 1655); defining an infinite set of solutions to a problem in number theory (Brouncker and Wallis 1657); and discovering infinite series for logarithmic and trigonometric quantities (Newton 1665). All of these topics were treated (though not always in their earliest manifestation) in *A treatise of algebra* and were at the time (as now) considered algebraic activities. What they have in common is in each case a movement from the concrete to the abstract, from the particular to the general. To do algebra, or to algebraicise, is to seek out

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<sup>802</sup> *Treatise of algebra*, 128, my italics.

<sup>803</sup> Van der Waerden 1985; Pycior 1997; Bashmakova and Smirnova 2000.

essential structures and eventually to represent them in concise and internally consistent notation which can be manipulated to reveal further insights, a definition as valid in the sixteenth and seventeenth centuries as now.

Wallis's four names, *analysis*, *al-jabr*, *regula cosae* and *ars magna*, had served well enough at the opening of *A treatise of algebra*: between them they adequately described the kind of algebra that had developed by about 1600 and with which Wallis began his studies in 1648. But by the end of the seventeenth century the last three names had fallen into disuse and *analysis* was taking on a new meaning arising from the algebra of infinite series created by Newton and others. Wallis, though, never revised his descriptions or tried to introduce new ones, and perhaps never saw the need to do so. Algebra remained for him what it had always been, a generalised arithmetic. For Wallis increasingly sophisticated arithmetic in turn necessitated new forms of algebra. Hence, among other things, his insight that rational, surd and imaginary numbers were enough to satisfy the finite equations of traditional algebra, but that the transcendental quantities which arose from the quadratures of the circle or hyperbola could only be expressed by the new algebra of infinite series.

For Wallis, applications of algebra to geometry were of secondary importance. It seems strange to say this of the mathematician who produced the first systematic algebraic formulation of conics, but Wallis's purpose in doing so was not primarily, as it might have been for Viète or Descartes, to investigate the *geometric* properties of curves, but to apply to them the *arithmetic* methods of the *Arithmetica infinitorum*. Newton, who shared Wallis's arithmetic approach to algebra, also appeared to share his view that algebra and geometry were best kept separate, but whereas Newton upheld a pure geometry unsullied by algebra,<sup>804</sup> Wallis was interested in 'pure Algebra, abstracted from Geometry'. Such concerns came rather too late, however, for Wallis and Newton had both played major roles in ensuring that not just geometry but every aspect of mathematics came to be handled algebraically.

The transition to algebra was considerably easier once a good general notation had gained wide acceptance, but before that (and again afterwards)

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<sup>804</sup> Fauvel 2000, 12.



notation lagged, often a long way, behind conceptual advances. Good notational ideas were sometimes lost (Chuquet's index notation), others spread slowly and unevenly (Recorde's 'equals' or Harriot's inequality signs), and false starts (cossist notation and Viète's  $A_{\text{quadratus}}$ ) hindered progress. Wallis noted many developments and improvements in notation, but in his own work often fell back on what he had first learned, the notation of Oughtred. In this as in so many ways the impact of Wallis's first encounter with algebra was discernible for the rest of his life.

### Wallis's perspective on history

The revitalisation of English mathematics can conveniently be dated from 1631 when the *Clavis* and the *Praxis* first made available to English readers some of the new work that had been coming to fruition in Italy and France. By the time Wallis wrote *A treatise of algebra* less than fifty years later, English mathematics was amongst the most advanced in Europe. Wallis's own career mirrored this astonishingly rapid pace of change: from a little self-taught algebra he rose to become one of the foremost mathematicians of his day. In writing a history of seventeenth-century mathematics he was in more than one way writing his own story.

The structure of *A treatise of algebra* often reflects this. The algebra in the book comes to life at the point where Wallis and so many of his contemporaries had started, with Oughtred's *Clavis*. Wallis's brief survey of algebra from al-Khwārizmī to Viète was necessary to set the scene, but lacked either substance or enthusiasm. Oughtred, Harriot and Pell were given ample space but the final quarter of the book was given over to the work arising from Wallis's own *Arithmetica infinitorum* and to the new mathematics which had arisen from it, culminating with results made public by Newton even as Wallis was completing his draft.

*A treatise of algebra* was about English mathematics, written with the intention of bringing English achievements to public notice. Wallis always had a keen sense of the benefits to both author and audience of publishing new ideas, and his efforts in this respect contributed significantly to the

contemporary mathematical scene. Aubrey once wrote bitterly of Wallis that:<sup>805</sup>

. . he lies at watch, at Sir Christopher Wren's discourse, Mr Robert Hooke's, Dr William Holder, &c; putts downe their notions in his note booke, and then prints it, without owneing the authors . . But though he does an injury to the inventors, he does good to learning, in publishing such curious notions, which the author (especially Sir Christopher Wren) might never have the leisure to write of himselfe'.

Aubrey's irony obscured a kinder truth: that Wallis did much to ensure that the work of other mathematicians (many less eminent than Wren) was published and correctly attributed. Work by Neile, Wren and Brouncker had already appeared, fully acknowledged, in Wallis's publications during the 1650s and *A treatise of algebra* gave Wallis a new opportunity to ensure that the work of mathematicians both well known and obscure reached a wider audience (see Appendix III).

Wallis saw publication as a means of proclaiming English success not only at home but abroad. His nationalism appears in striking contrast to the spirit that had prevailed in earlier centuries when western Europe had shared a common mathematical culture which moved easily across national boundaries (see thesis Chapters 2 and 3) but Wallis's view was not untypical. Among Pell's papers are several copies of a 1678 pamphlet<sup>806</sup> which advertised the forthcoming *English atlas* (then being compiled with the assistance of Wren, Pell and Hooke),<sup>807</sup> and the pamphlet stated that 'The Work is intended for the credit of our Nation', just the kind of language that Wallis was also using (and Pell could well have been one of those who encouraged him). An analysis of seventeenth-century nationalism is beyond the scope of this thesis,<sup>808</sup> but it was

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<sup>805</sup> Aubrey 1898, 281-282.

<sup>806</sup> British Library Add MS 4394, f. 405 and elsewhere. Pell used the blank spaces in these pamphlets for his calculations.

<sup>807</sup> Pitt 1680-83.

<sup>808</sup> Pycior 1997, 111, speaks of the rise of English nationality, but a sense of nationality and nationalism are not the same thing; English nationality was established long before the seventeenth century.

not a purely English phenomenon, for Fermat had been more provocative in this respect than Wallis.

It was unfortunate for the later reception of *A treatise of algebra* that Wallis's efforts on behalf of his countrymen were so often marred by contempt for foreigners, especially the French. Wallis's dislike of the French must have sprung in part from his mathematical quarrels in the late 1650s, first with Fermat, then with Pascal who had behaved less than honourably towards both Wallis and Lalouvière over their work on the cycloid.<sup>809</sup> (Antoine Lalouvière was one of the few Frenchmen of whom Wallis ever afterward spoke with some regard.) Two lesser but bitterly fought disputes in 1667-68 with Francis Dulaurens,<sup>810</sup> and Vincent Leotaud (see Appendix III) would have further inflamed his prejudices.

Wallis's mistrust, however, was rooted in more than such personal quarrels: through his work as a code-breaker he was all too familiar with the potential for political treachery. William Wallis, a great-great-grandson of the mathematician, later wrote with perception (if not punctuation) of Wallis's knowledge of foreign affairs:<sup>811</sup>

He must likewise have been well acquainted with what was passing in the several courts and countries in Europe for without this knowledge it must have been impossible to have explained many passages where only a hint was given and which was often enough to explain the writers meaning to the person wrote to who may be supposed to know something of the business but to a third person must without that knowledge have appeared unmeaning and unintelligible.

Wallis, in common with many others, was particularly suspicious of the intentions of Catholic France. (One of the very first letters he had deciphered was from the Catholic sympathiser Francis Windebank, whom Wallis would

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<sup>809</sup> Pascal 1658 and 1659; Wallis 1659; Lalouvière 1658 and 1660; Tatton 1974, 336, 339.

<sup>810</sup> The quarrel with Dulaurens arose over 'Simon de Montfert's problem'. Solutions by Moore (1658) and Wren (undated) are preserved in MS Aubrey 10. See Wallis 1658, letter no. 40; Dulaurens 1667, 249; Wallis 1668a,b,c. There are other letters relating to the affair throughout Hall and Hall 1965-86, vols IV and V.

<sup>811</sup> William Wallis, MS Eng. misc. e. 475, ff. 275-276.

have regarded as a traitor and who had fled to France.)<sup>812</sup> His perceptions of the dangers of Catholic domination are illustrated in a letter (previously unpublished) to his friend Thomas Smith in 1698:<sup>813</sup>

I concur with you in considering the hardships of the Greek church under the Turkish oppression. And heartily wish them a more happy condition. But if they should change the Turkish slavery for that of the Romist I doubt they would change for the worse. For, certainly, the Protestants in Hungary are in much worse circumstances, under the Christian Emperor, than they were under the Turkish. And like oppressions there are in Poland, France and elsewhere, especially where the Jesuits rule.

Against such a background it is hardly surprising that Wallis had few good things to say of France or its mathematicians.

Wallis was at his best as a historian when he focused not on nationalities or personalities but on mathematics. This was so even in his much criticised treatment of Harriot once he forgot to harangue Descartes and concentrated instead on the implications of Harriot's algebra. His historiographical skills were at their most refined in his study of Hindu-Arabic numerals where, with no English hobby-horse to ride, his evidence was thoroughly researched and carefully considered. But Wallis went beyond the introduction of new techniques: he redefined the way the history of mathematics was understood and written. To the medieval mind mathematics was ancient and given knowledge handed down from one generation or civilisation to the next, and all histories of mathematics up to the end of the sixteenth century reflected this view. By the seventeenth century the pace of change in mathematics (as in other subjects) rendered such ideas untenable and history had to be written differently. Wallis was the first to present mathematics as a system of living, changing ideas which evolved and spread in complex and not always easily discernible ways. This new approach is first and perhaps most strongly evident in his discussion of the transmission of the Hindu-Arabic numeral system, but it also runs as an underlying assumption throughout his text. The title gives it

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<sup>812</sup> Scriba 1970, 38.

<sup>813</sup> Wallis to Smith 21 December 1698, MS Smith 54, f. 55.

away: ‘ . . shewing the Original, Progress and Advancement thereof, from time to time; and by what Steps it hath attained to the Heighth at which now it is’.

Wallis’s first hand knowledge of mathematics and mathematicians, his access to Oxford’s rich resources, and his own mathematical and linguistic skills all contributed to his writing of the first modern history of algebra. To many of its readers his book has appeared partial and eventually outdated, but in its time it was a new kind of work which, for all its weaknesses, established the history of mathematics as a subject for serious discussion; indeed the very inadequacies of Wallis’s account provoked and maintained such discussion for years afterwards. Now, three centuries after it was written, it is possible to read Wallis’s book afresh, to enjoy its riches and see its shortcomings not as failures, but as valuable indications of the motives and concerns of Wallis and his contemporaries. *A treatise of algebra* provides a unique insight into seventeenth-century mathematics through the eyes of one of its foremost practitioners, and in this it is of inestimable value. This thesis has done no more than explore avenues signposted by Wallis in the pages of his treatise, and is both testimony and tribute to the lasting value of his work.



## Appendices

### **Appendix I : Seventeenth-century Bodleian Library collections containing medieval mathematical material**

#### *Savile collection (1619)*

When Henry Savile (1549-1622), Warden of Merton College, founded the Oxford chairs of geometry and astronomy in 1619 he also donated his personal collection of mathematical books, notes and manuscripts for the use of the Savilian professors. The original collections consisted mainly of sixteenth-century printed texts and about forty handwritten volumes on mathematics and astronomy in Greek or Latin including a few important volumes of medieval texts. All the seventeenth-century professors added generously to the Savile Library, making it the best collection in England, perhaps anywhere, of mathematical texts up to 1700.<sup>814</sup> For many years it was housed in the tower between the Schools of geometry and astronomy (where the Lower Reading Room reserve desk now stands); Wallis knew it thoroughly and his annotations are to be found frequently in both its books and its manuscripts. The Savile Library was incorporated into the main Bodleian Library in the nineteenth century.

#### *Digby collection (1634)*

The second great collection of mathematical manuscripts came from Sir Kenelm Digby (1603-1665), then a naval commander, later a diplomat, who was encouraged to donate it by Sir William Laud, Archbishop of Canterbury and Chancellor of the University of Oxford from 1629 to 1645. Over half of Digby's collection had been bequeathed to him by his old tutor, Thomas Allen (1542-1632), a mathematician of Trinity College and later Gloucester Hall. Allen had rescued some of the mathematical texts which Merton College was forced to dispose of after the Reformation, making the Digby collection a particularly rich source of medieval mathematics. Of its 238 volumes at least 40 contain medieval mathematical texts, making it by far the richest single

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<sup>814</sup> The first catalogue of the Savile collection was Bernard 1697.

collection of such material in England. The Digby collection has its own catalogue,<sup>815</sup> and the contents are not included in the Bodleian Library's *Summary catalogue of western manuscripts*.

*Laud collection (1635-1640)*

Following his encouragement to Digby, William Laud (1573-1645) gave his own collection to the Bodleian Library in four donations between 1635 and 1640, almost doubling the library's existing holdings. Greek mathematical texts were acquired for Laud by John Greaves, later Savilian Professor of Astronomy (1643-1649) who travelled to Constantinople on Laud's behalf in 1637.<sup>816</sup>

*Selden collection (1659)*

The Selden collection was the legacy of jurist John Selden (1554-1654). The manuscripts are mainly of Greek and oriental origin, but there are also a few important medieval Latin texts. The majority of Arabic and Persian texts were acquired from the estate of John Greaves after his death in 1652.

*Ashmole collection (1683)*

The Ashmole collection was acquired by the Bodleian Library later than the others, in 1683, when Wallis had already completed *A treatise of algebra*, but it too included mathematical texts and there is some reason to suppose that Wallis consulted it. It was the personal collection of Elias Ashmole (1617-1692) and reflected his special interest in alchemy and astronomy. The original Ashmolean museum was built next door to the Bodleian Library (where it is now the Museum for the History of Science) to house Ashmole's collection of 'curiosities' acquired from John Tradescant in 1659; it opened in 1683 and held the manuscripts until they were transferred to the main library in 1860.<sup>817</sup>

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<sup>815</sup> Macray 1883.

<sup>816</sup> Coxe 1853b.

<sup>817</sup> Black 1845.



## Appendix II : English sources used by Vossius

For his English writers Vossius drew especially on the work of three sixteenth-century English historians: John Leland, John Bale and John Pits.

*John Leland* (c.1506-1522), born in London, was educated at Christ's College, Cambridge and All Souls, Oxford, and in 1530 became chaplain and library keeper to Henry VIII. In 1533 he was made 'King's Antiquary', a special appointment never made before or since, and was commissioned to search out manuscripts and artefacts in the monasteries and colleges of England, many of which were about to be closed. Leland spent the best part of the next ten years on the work and presented an account of his journey to Henry in 1545.<sup>818</sup> He planned a full account of early English writers, but it was never published in his lifetime; he became insane and died in London in 1552. The notes of his findings, however, his *Collecteana* and *Itinerary*, were circulated, copied and used by many later historians, and the originals were eventually acquired by the Bodleian Library in 1632.<sup>819</sup> They were first edited and published by Bodleian Librarian Thomas Hearne as *Itinerary of John Leland the antiquary* in 1710 and *Collecteana* in 1715.

Leland's contemporary *John Bale* (1495-1563) began his education at the Carmelite monastery in Norwich, followed by Jesus College, Cambridge. Initially a zealous Catholic, he converted and turned to writing virulent attacks on the Catholic church, earning himself the nickname of 'bilious Bale', and from 1540 spent seven years in exile in Germany. After his return to England he began to keep a detailed notebook of the names, biographical details and works of English writers, drawing freely on the earlier findings of Leland as well as his own research. Bale was perhaps particularly familiar with writers from his own East Anglian background, and his researches help to account for the sprinkling of East Anglian names in Wallis's list: John Baconthorp, Nicholas of Lynn and Richard Lavenham who, like Bale, were all Carmelites, as well as the Norfolk antiquary William Botoner. Bale published two major

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<sup>818</sup> Leland 1549.

<sup>819</sup> MS 5102-5106; MS 5107-5112.

works: his *Summarium* in 1548 and his *Catalogus* in 1557-59.<sup>820</sup> His notebook was eventually acquired by John Selden and given to the Bodleian Library as part of the Selden collection, and its contents were published as *Index Britannia scriptorum* in 1902.<sup>821</sup>

Bale's work was later taken up by *John Pits* (1560-1616), who was educated at New College, Oxford, but spent most of his life in France and Bavaria. Pits' accounts of English writers in his *Relationum historicarum de rebus Anglicis* of 1619 were closely based on those in Bale's *Summarium*, though Pits greatly disliked Bale and tried to redress his religious imbalance back towards Catholicism.

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<sup>820</sup> Bale 1548; Bale 1557-59.

<sup>821</sup> MS Selden Supra 64; Bale 1902.

### Appendix III : *A treatise of algebra*, appendices and additions

#### Appendices

There are four appendices to *A treatise of algebra*, adding half as much again to the length of the book. All were written between 1662 and 1672, and were probably the papers Collins had been holding for Wallis awaiting publication. All the appendices were translated into Latin and republished, in a slightly different order, in 1693.<sup>822</sup>

i) *Cono-cuneus or the shipwright's circular wedge* (17 pages)<sup>823</sup>

A treatise written by Wallis in 1662 to consider shapes circular at the base, wedge shaped at the top, of potential use in ship building.

ii) *Treatise of angular sections* (69 pages)<sup>824</sup>

Begun by Wallis in 1648 after reading Oughtred's *Clavis*, and completed in 1665 (see thesis Chapter 4).

iii) *A defense of the treatise of the angle of contact* (36 pages)<sup>825</sup>

A discussion of the dispute between Peletier and Clavius arising from Euclid III.16, on whether the angle between a circle and its tangent at the point of contact could be said to have any magnitude. Wallis sided with Peletier whose view was that it did not.<sup>826</sup> Wallis had written his original treatise on this subject in 1656, and he took up cudgels again after Leotaud in his *Cyclomathia* of 1662 came out in favour of Clavius.<sup>827</sup> A long letter from Wallis to Leotaud written in 1667 forms the greater part of Wallis's new *Defense*.<sup>828</sup> The penultimate chapter also contains an interesting discussion of

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<sup>822</sup> Wallis 1693, 483-704.

<sup>823</sup> *Treatise of algebra*, first appendix, paginated 1-17 plus diagrams.

<sup>824</sup> *Treatise of algebra*, second appendix, paginated 1-69.

<sup>825</sup> *Treatise of algebra*, third appendix, paginated 70-105.

<sup>826</sup> Peletier 1557, 'Preface' and 73-76; Vitellio 1572, 18; Clavius 1574, 132f; Heath 1908, II, 39-43.

<sup>827</sup> See also Aynscom 1656; Tacquet 1669, also mentioned by Wallis.

<sup>828</sup> Wallis to Leotaud, 17 February 1668, *Defense*, 79-88; Wallis 1693, 638-645.

magnitudes which ‘are nothing; yet are in the next possibility of being somewhat . . . And may very well be called *Inchoatius* or *Inceptives* of that somewhat to which they are in such possibility,’<sup>829</sup> an idea which in some ways prefigured later discussions on the nature of fluxions.

iv) *A discourse of combinations, alternations and aliquot parts* (47 pages)<sup>830</sup>

This treatise was first mentioned in Wallis’s discussion of Harriot, in Chapter 37 entitled ‘The composition of coefficients’, where having noted how the coefficients of polynomial equations were composed from the roots Wallis set out to detail the permitted combinations. The first three chapters of the *Discourse* are one of the few parts of *A treatise of algebra* for which we have a handwritten version: they are found in Latin at the back of Wallis’s mathematical notebook,<sup>831</sup> and the heading says that they were written in 1672 and ‘transcribed hither Dec.24.1674’. The three chapters are unaltered (except for translation into English) in *A treatise of algebra*, but Wallis could not resist adding to his work even as he translated it and inserted extra material at the end of each chapter. One insertion concerns an explanation of a rule given by Buckley in his *Arithmetica* of 1577, another gives an answer to combinatorial problems posed by Vossius.<sup>832</sup> The fourth and final chapter contains Wallis’s work on one of the number problems put to him by Fermat in 1657, in modern notation, to solve  $x^3 + x^2 + x + 1 = y^2$  and  $x^2 + x + 1 = y^3$  in integers (see thesis Chapter 8). Both he and Frenicle had long ago found several solutions,<sup>833</sup> but Wallis’s anger was aroused by the posthumous publication of Fermat’s work in 1679 which reprinted the challenge, specifically addressed to Wallis, but without either Wallis’s solutions or Fermat’s acknowledgment of them. Wallis reworked both problems in greater detail than in 1658 and this time made sure by adding them to his own book that his achievement was publicly recognised.

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<sup>829</sup> *Defense*, 96; Wallis 1693, 653.

<sup>830</sup> *Treatise of algebra*, fourth appendix, paginated 106-152.

<sup>831</sup> MS Don. d. 45, f. 260.

<sup>832</sup> Vossius 1650, 28-29. Wallis’s copy, Savile G.21, is well-thumbed and heavily annotated at this point, and a half sheet of Wallis’s working is inserted between the pages.

<sup>833</sup> Frenicle 1657.

## Additions and emendations

The final twenty-four pages of *A treatise of algebra* are the ‘Additions and Emendations’ added while the book was in press.<sup>834</sup> Much of the material they contain has already been discussed: medieval numerals and the biography of Gerbert (thesis Chapter 2)<sup>835</sup> and Wallis’s late discovery of Recorde’s *Whetstone of witte* (thesis Chapter 3).<sup>836</sup> The following additions were all incorporated into the main text when *A treatise of algebra* was republished in 1693:

### *Merry’s treatise* (6 pages)<sup>837</sup>

An excerpt from a treatise written by Merry, showing how polynomials with one or more zero coefficients should be factorised, essentially an explanation of rules already devised by the Dutch mathematician Hudde. The full treatise ran to 236 pages in manuscript, and at one time Wallis had suggested that it should be published, alone or with Kersey’s *Elements*,<sup>838</sup> but the plan had come to nothing. Now Wallis copied a short extract and noted that he had placed the manuscript in the Savile Library ‘in case any shall think fit to Print it’ (Merry himself had died in the meantime). The manuscript is still there, and has never been published.<sup>839</sup> Perhaps the most interesting thing about it is that is written throughout in Pell’s three-column style.

### *A letter from Thomas Strode* (5 pages)<sup>840</sup>

A geometrical construction for quadratic equations sent to Wallis in November 1684 by Thomas Strode (fl.1642-1688) of University College, Oxford, together with Wallis’s reply. Wallis’s letter is most remarkable for the fact that as late as 1684 he wrote it entirely in Oughtred’s notation.

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<sup>834</sup> *Treatise of algebra*, following the appendices, paginated 153-176.

<sup>835</sup> *Additions and emendations*, 152-157.

<sup>836</sup> *Additions and emendations*, 157.

<sup>837</sup> *Additions and emendations*, 157-162.

<sup>838</sup> Wallis to Collins, 15 March 1673; Wallis to Collins, 29 March 1673, Rigaud 1841, II, 554-555, 560.

<sup>839</sup> MS Savile 33.

<sup>840</sup> *Additions and emendations*, 162-166.

*A paper by John Caswell* (6 pages)<sup>841</sup>

An application of some of the principles of the *Arithmetica infinitorum* to geometry by John Caswell (1656-1712) of Wadham College (a candidate for the Savilian professorship of astronomy in 1693 and eventually appointed to that position in 1708).

*Trigonometrical relationships* (1 page)<sup>842</sup>

A page by Wallis originally written at the request of John Collins, outlining the relationships between various trigonometric quantities.

*A letter from George Fairfax* (3 pages)<sup>843</sup>

A geometrical problem and Wallis's reply.

*A note on Caswell's account of trigonometry*<sup>844</sup>

A brief note by Wallis pointing out that Caswell's trigonometry was printed alongside *A treatise of algebra*.

*Other emendations* (2 pages)<sup>845</sup>

Two pages of typographical corrections, followed by a final sentence on the outcome of Wallis's researches at Bristol: 'they find the inscription but not the Date'.

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<sup>841</sup> *Additions and emendations*, 166-171.

<sup>842</sup> *Additions and emendations*, 171-172, originally in Wallis to Collins, 8 June 1672, Rigaud 1841, II, 537-539.

<sup>843</sup> *Additions and emendations*, 172-174.

<sup>844</sup> *Additions and emendations*, 174.

<sup>845</sup> *Additions and emendations*, 175-176.

#### Appendix IV : Revisions and additions in 1693

When *A treatise of algebra* was republished in Latin in 1693 as part of his *Opera mathematica* Wallis made good use of the opportunity to add new material though little of it, as he admitted, had any bearing on the history of algebra. In 1685 he had relegated such material to appendices, but in 1693 he simply added thirteen extra chapters (almost 100 pages) to the end of his book. Of greater interest, however, are the new paragraphs inserted earlier in the text, several of which appear to have been written in response to criticism of the original. The most significant of these, particularly those relevant to the content of this thesis, are listed here (with page numbers in the 1685 edition in square brackets). I have also noted some of the handwritten corrections that Wallis made in his own copy of the 1693 translation (Savile Gg.2).

*Preface* [new] : In a tone of justification more than apology Wallis acknowledged that, despite its title, *A treatise of algebra* was rather more historical than practical. For technique Wallis recommended other authors: Viète, Oughtred, Harriot, Descartes, Van Schooten, Sluse and Kersey. He had all but completely ignored Kersey in 1685 but now described his work as ‘copiously and clearly set out’ and added that no-one had better elucidated the problems of Diophantus.

*Following the preface* [new] : *De Harrioto Addenda* in which Wallis, goaded by Baillet’s remarks, finally revealed Pell as the source of his information on Harriot.

*p.8* [8] : Seven additional paragraphs on Greek and Hebrew alphabetic numerals (see thesis Chapter 2, § 3.5).

*p.15* [15] : Additional material on the lost Bristol inscription and Luffkin’s letter on the supposed date of 1090 on a Colchester window (see thesis Chapter 2).

*p.63* [61] : Here Wallis claimed that Harriot had begun to work on antilogarithms and that Warner happened to come across the papers, or so Pell had lately informed him (‘lately’ is open to interpretation since Pell had died eight years earlier). Wallis recalled seeing such papers (and, he added, only those) among papers of either Harriot or Warner over thirty years ago. What happened to them after that he did not know, until he found out from Pell that

they were with Busby at Westminster. He had hoped Pell would complete them, but Pell had died before he could do so.

p.67-68 [63] : After pointing out (as in 1685) that Bombelli knew how to reduce a biquadratic by way of a cubic, Wallis added a note that Bombelli was, so far as he knew, the first to do so. As though to emphasise Bombelli's achievement, he mentioned it again two paragraphs later. It seems that between 1685 and 1693 Wallis took a greater interest in Bombelli's work but remained ignorant of the earlier work of Cardano. He was, however, perhaps now more aware of shortcomings in his review of sixteenth-century texts for he also added a note of apology for any authors he had forgotten or not seen.

p.77 [73] : When it came to Oughtred, readers could be forgiven for complaining not of too little detail but too much. Wallis gave two justifications for expounding Oughtred's rules at such length: first, because they were set out so briefly, though admirably so, in the *Clavis* itself; second, because, as in any learning, the rules are better remembered if the reasons are fully understood. These somewhat contradictory attitudes to Oughtred's brevity betray the fact that Wallis's real concern was something different, the promotion of the *Clavis*, which, at his instigation, was about to go into its fifth edition. In fact his exposition of Oughtred was even longer in 1693 than in 1685.

p.135 [121] : Wallis claimed that he still had by him ('*apud me adhuc habeo*') a letter from John Smith, dated 28 November 1648, urging him to publish his early results. Neither this nor any other of Wallis's 1648 correspondence with Smith, however, has ever come to light.

p. 136 [126] : There is a small but significant change here. In 1685 Wallis had written: '[Harriot] laid the foundation on which *Des Cartes* (though without naming him) hath built the greatest part (if not the whole) of his *Algebra* or *Geometry*. Without which, that whole Superstructure of *Des Cartes* (I doubt) had never been.' In 1693 this was replaced by: '[Harriot] laid the foundations on which Descartes built if not all, certainly the greater part of his *Algebra* and *Geometry* (remaining silent about Harriot); which work he first published in 1637, at least in French.' The contentious final sentence has disappeared.



p. 187 [177] : Wallis once again displayed his ignorance of Cardano's work when on the subject of cubic equations he wrote: 'Cardano (if I remember correctly) teaches nothing about the removal of the second term (nor do I know of anyone before Harriot)'. Cardano had in fact pioneered such methods, in which Bombelli, Stevin and Viète had all followed him, but Wallis's admiration of Harriot blinded him to all earlier achievement. As to the solution of cubics, Wallis claimed even now in 1693, that his 1648 method using Oughtred's notation was still the simplest and clearest available.

p. 206-217 [200] : After listing Harriot's improvements in algebra Wallis added his correspondence with Morland on the subject of Descartes' supposed plagiarism. Wallis denied ever having called Descartes a plagiarist ('*verum ego Plagii nomen Cartesio nusquam impingo*') but followed this with a page-by-page analysis of *La Géométrie* indicating where Descartes might have found similar work in previous authors: Viète, Ghetaldi, Oughtred, Harriot, Bombelli, Stevin, Hérigone, Cardano, Ferrari and Ferro. Wallis then added another long letter in response to Prestet (Malebranche) who in the second edition of his *Nouveaux elemens* had poured scorn on Wallis's assertions about Descartes (see thesis Chapter 9).<sup>846</sup> Wallis held fast to Harriot's priority:<sup>847</sup>

Whether Descartes read Harriot or did not read him or did not read all of him, or took things from others who had read him, or someone added to his writing those things which were in common with Harriot, or from whatsoever place the fact arises, equal things are to be had in both; it is certain that those things were first taught by Harriot before Descartes, and there is no wrong on my part that I have stated this to be the case.

p.228 [208] : Here, on the subject of biquadratics, was the third incorrect assertion that Bombelli was first to solve biquadratics by way of a cubic. Wallis even quoted the relevant page number in *L'algebra*. He added that Viète had used the same method but that he, Wallis, knew no other ('*nescio an alii*'). Cardano was still missing. Wallis was now more careful to acknowledge Descartes' improvement to the method and omitted the rather disparaging

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<sup>846</sup> Prestet 1689, II, Preface (unpaginated).

<sup>847</sup> Wallis 1693, 217.

phrases he had previously used, but maintained, nevertheless, that Descartes was following the precepts laid down by Harriot.

p. 233 [213] : Wallis's original list of mathematicians 'amongst ourselves' who had made contributions to algebra had included Barrow, Brouncker, Newton, James Gregory (English by association), Mercator (English by assimilation), Kersey and Dary. To these he now added Warner, Ward, Wren, Bernard, David Gregory, Caswell, Pell, Scarbrough, Davenant, Collins, Halley, Strobe, Raphson, Adam Martindale (preacher and schoolmaster), Clark and N. Hanbury (unknown) as well as William Molineux (FRS and MP for Dublin University) and St George Ash (unknown) in Dublin. Wallis's original list of foreign writers had included the few he admired even though, he admitted, they had done little or nothing in algebra: Mydorge, St Vincent, Lalouvière, Bouillau, Pascal (despite the quarrels), Viviani and Sluse. In 1685 Prestet had also been in this select group but after his 1689 attack on Wallis he was dropped.

p. 236 [216] : On the subject of indeterminate equations Wallis added yet another accolade (the third) to Kersey for his treatment of Diophantus in the second volume of his *Elements*.

p. 287 [266] and p. 289 [267] : On imaginary numbers, Wallis noted Harriot's use of  $\sqrt{-ddddd}$  and Bombelli's use of *piu di meno*, another sign of his increased familiarity with Bombelli's work.

p. 359 [320] : The sequence of coefficients in the expansion of  $(1 + x)^{1/2}$  had been wrongly written in 1685 as :

$$1. \frac{1}{2} \cdot \frac{-1}{2 \times 4} \cdot \frac{+3}{2 \times 4 \times 6} \cdot \frac{-5}{2 \times 4 \times 6 \times 8} \cdot \frac{+7}{2 \times 4 \times 6 \times 8 \times 10}$$

The error stood uncorrected in 1693, one of the rare occasions when Wallis's sense of pattern had led him astray. In Wallis's copy (Savile Gg.2), however, a sheet has been inserted (though not in his handwriting) with the correct figures:

$$1. \frac{1}{2} \cdot \frac{-1}{2 \times 4} \cdot \frac{+1 \times 3}{2 \times 4 \times 6} \cdot \frac{-1 \times 3 \times 5}{2 \times 4 \times 6 \times 8} \cdot \frac{+1 \times 3 \times 5 \times 7}{2 \times 4 \times 6 \times 8 \times 10} \text{ \&c.}$$

p. 368 [330] : The date of Newton's *Epistola posterior* was wrongly stated in 1693 to be 24 August 1676, though the true date, 24 October 1676, had appeared in 1685. (Wallis gave the same erroneous date when writing to

Newton and to Waller in 1695.)<sup>848</sup> The mistake was corrected by hand in Savile Gg.2. In 1699 Wallis added a further handwritten note to inform readers that Newton's letters appeared in full in Volume III of his collected works, published that same year.

*p. 377-380 [338]* : At the end of his chapter on the quadrature of the hyperbola Wallis inserted a letter from David Gregory (by now Savilian Professor of Astronomy) describing his method of quadrature.

*p. 389 [347]* : Following Leibniz' series for  $\pi/4$  ( $= 1 + \frac{1}{3} - \frac{1}{5} + \frac{1}{7} + \dots$ ) Wallis added a series found by Newton for  $\pi/(2\sqrt{2})$  ( $= 1 + \frac{1}{3} - \frac{1}{5} - \frac{1}{7} + \dots$ ). Newton had sent him this new series in August 1692.<sup>849</sup> Wallis left it to the reader, however, to decide whether either of these was better than his own, discovered, he pointed out, before any of them.

*p. 390 [347]* : At the end of this chapter on Newton's results on infinite series Wallis said he had hoped, even in 1685, that Newton would publish more of his work. He had indeed done his best to persuade Newton to be more forthcoming. In August 1692 he wrote the following letter which is not printed in the standard edition of Newton's correspondence and so is given here in full:<sup>850</sup>

Aug.13.1692

Sir

I thought fit to acquaint you that I am now printing my Algebra in Latine, here at Oxford. Alone three score sheets are already printed, & we go on apace, about 5 sheets a week. We shal quickly come to that part which concerns you; wherein I shal be willing to do you all the right I can. What you think fit to have added, altered or amended, in what concerns you; I

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<sup>848</sup> Wallis to Newton, 10 April 1695; Wallis to Waller, 30 April 1695, Turnbull 1959-77, IV, 100, 115.

<sup>849</sup> Newton to Wallis, 27 August 1692, no. 392 in Turnbull 1959-77, III, 219.

<sup>850</sup> Wallis to Newton, 13 August 1692, original never found. A copy in Wallis's hand was discovered in the Centre for Kentish Studies, Maidstone, U 120 F.15, by Philip Beeley and will be included in the forthcoming edition of Wallis's correspondence edited by Scriba and Beeley.

shal therein observe your directions. Particularly, I desire you favour me with the two methods intimated, chap. 95. pag. 345, *the one more Ready, the other more General*; which, with your leave, I shal insert in their due place. And if the Printer have by mistake committed some Errors (of which you may be sooner aware than I) you may please to signify them, what else you think fit to as soon as with convenience you can, lesst they come too late,) to

Sir

Your very humble servant

John Wallis

In reply Newton sent Wallis two letters dated 27 August and 17 September on his method of fluxions, and Wallis printed both.<sup>851</sup> He clearly wished that Newton would have sent him more but decided to print what he had, lest it go to waste (*'ne pereant'*). Even this was no small achievement: it was the first time that Newton's method of fluxions and dot notation had appeared in print, and was all that was available until Joseph Raphson published more in his *Historia fluxionum* of 1715.<sup>852</sup>

The original letters have not been found but Turnbull, editor of Newton's correspondence, has reconstructed them on the basis of Wallis's text.<sup>853</sup> It is possible, however, to argue for a slightly different composition than the one Turnbull suggested. The opening of Turnbull's 27 August letter is in fact material from the *Epistola posterior* of 1676,<sup>854</sup> and it is perhaps more plausible that the letter of 27 August opened with the new material which begins: *Si Curvae abscissa sit z . . .* (If  $z$  be the abscissa of a curve . . .). This is immediately followed in Wallis's text by Problem I, which Newton himself said he sent on 27 August.<sup>855</sup> (Turnbull, despite his footnote on Newton's

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<sup>851</sup> Wallis 1693, 391-396.

<sup>852</sup> Raphson 1715.

<sup>853</sup> Nos. 393, 394 in Turnbull 1959-77, III, 222-228.

<sup>854</sup> Wallis 1693, 390-391; Turnbull, II, 134-136.

<sup>855</sup> Turnbull, III, 229 n. 9.

dating, put Problem I into his reconstructed letter of 17 September.<sup>856</sup>) It seems reasonable to suppose that the letter of 27 August ended *after* Problem I with the words *reciproce proportionales* halfway down Wallis's page 393. It would then make sense for Wallis to write the linking paragraph that appears there before copying Problem II (beginning *Haec methodus . . .*) from the second letter, of 17 September.

Wallis later regretted that he had not included the entire contents of the *Epistola prior* and *Epistola posterior* in the 1693 translation.<sup>857</sup> He complained to Newton, perhaps more bitterly than anyone else would have dared, about his reluctance to publish:<sup>858</sup>

For, I suppose, your other friends call upon you for it, as well as I; & are as little satisfied with the delay. . Consider, that 'tis now about Thirty years since you were master of those notions about *Fluxions* and *Infinite Series*; but you have never published ought of it to this day. 'Tis true, I have endeavoured to do you right in that point. . . And even what I have said, is but playing an After-game for you; to recover (precariously *ex postliminio*) what you had let slip in its due time. And, even yet I see you make not great hast to publish those Letters, which are to be my Vouchers for what I say of it. And even those letters at first, were rather extorted from you, than purely voluntary.

In the end Wallis took matter into his own hands and in 1697 he wrote to Newton:<sup>859</sup>

Those two letters of yours (rather long and full of matter) written in 1676 (excerpts from which I have already published) I shall take care (unless you forbid it) to subjoin to a certain book of mine (now for quite a long time in the press) as soon as printing delays allow.

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<sup>856</sup> Turnbull, III, 223-225.

<sup>857</sup> Wallis to Newton, 10 April 1695; Wallis to Waller 30 April 1695, nos. 498, 502 in Turnbull, IV, 100-101, 114-115.

<sup>858</sup> Wallis to Newton, 30 April 1695, no. 503 in Turnbull, IV, 116-72; see also nos. 511, 518, 519.

<sup>859</sup> Wallis to Newton, 1 July 1697, no. 567 in Turnbull, IV, 238-239.

The letters finally appeared in full in the third volume of Wallis's collected works, fourteen years after the first extracts had appeared in *A treatise of algebra*.<sup>860</sup>

p. 396-397 [new] : The three pages on Newton's method of fluxions were followed by an extract from Raphson's *Analysis* of 1690 on the numerical solution of equations. A copy of the *Analysis*, with a warm handwritten dedication from Raphson to Wallis is now in the Savile Library.<sup>861</sup>

p. 398-402 [new] : Wallis's method of tangents previously published in the *Philosophical transactions* in 1672.

p. 418 [363] : Wallis here inserted Fermat's first challenge in full but omitted the second, so that Brouncker's response is left as a solution to an unstated problem.

p. 429-482 [new] : Chapters 100-112 are a collection of miscellaneous material, on nautical charts, the measurement of resistance in the motion of projectiles, why the sun and moon appear larger near the horizon, the Julian calendar, musical chords, parallax, how to pass a smaller cube through a larger, and the *annulis* puzzle from Cardano's *subtilitate*.

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<sup>860</sup> Turnbull, III, 220 n. 4, states that Wallis in 1685 had printed extracts from the *Epistola prior* but nothing from the *Epistola posterior*: this is incorrect. Wallis had reproduced the whole of the first letter and some relevant results from the second.

<sup>861</sup> Savile G.1.

## Tables

**Table 1: Cossist algebra texts 1494 – 1600**

The following table lists the known cossist algebra texts published up to 1600. The table has been compiled with the help of Rider 1982, but where possible I have examined at least one edition of each work and have excluded those which contain a little algebraic notation but which are strictly arithmetics rather than algebras. Texts I have been unable to inspect in Oxford or elsewhere are marked with †. (Authors not represented in Oxford are generally those who wrote in foreign vernacular languages: a Latin title does not always imply that the text itself is in Latin).

During the seventeenth-century the Savile Library was kept in the study of the Savilian professors but was later incorporated into the main Bodleian Library. All the books marked 'Savile' in the table were listed in the 1697 Savile Library catalogue and still carry 'Savile' as part of their shelfmark. Wallis's use of both the Savile and Bodleian collections is indicated in the final column.

Date first published	Author	Short title	Imprint	Availability in Oxford	Mentioned by Wallis
1494	Pacioli	<i>Summa</i>	Venice	Bodleian	*
1518	Schreiber	<i>Ayn new kunstlich Buech</i>	Nuremberg		
1520	de la Roche <sup>†</sup>	<i>L'arithmetique</i>	Lyons		*
1521	Ghaligai	<i>Summa</i>	Florence	Savile (1552)	*
1525	Rudolff	<i>Behend und Hubsch rechnung</i>	Strasbourg		
1526	Feliciano	<i>Libri</i>	Venice	Bodleian	
1544	Stifel	<i>Arithmetica integra</i>	Nuremberg	Savile	*
1551	Scheubel	<i>Algebrae compendiosa</i>	Paris	Bodleian	*
1552	Aurel	<i>Libro . . algebratica</i>	Valencia	Bodleian	
1554	Peletier	<i>L'algebre</i>	Lyons	Savile	*
1555	Dunus <sup>†</sup>	<i>Liber de arithmetica</i>	Basel		
1556	Mennher	<i>Arithmetique seconde</i>	Antwerp	Bodleian	
1556	Peucer	<i>Logistica</i>	Wittenberg	Bodleian	
1557	Reorde	<i>The whetstone of witte</i>	London	Savile	*
1559	Buteo	<i>Logistica</i>	Lyons	Savile	*
1560	Ramus	<i>Arithmeticae</i>	Paris	Savile	*
1562	Perez de Moya <sup>†</sup>	<i>Arithmetica</i>	Salamanca		
1564	Núñez	<i>Libro de algebra</i>	Antwerp	Savile	*
1573	Perez de Moya	<i>Tratado</i>	Alcala	Bodleian	
1575	Brucaeus <sup>†</sup>	<i>Mathematicarum exercitationum</i>	Rostock		
1577	Gosselin	<i>De arte magna</i>	Paris		
1579	Digges	<i>Stratoticos</i>	London	Savile	*
1580	Salinac	<i>Arithmeticae</i>	Frankfurt	Savile	*
1583	Lochner <sup>†</sup>	<i>Tractatlein</i>	Nuremberg		
1583	Petri <sup>†</sup>	<i>Practique om te leeren reekenen</i>	Amsterdam		
1589	Unicorn	<i>De l'arithmetica</i>	Venice	Bodleian	
1599	Coutereels	<i>Arithmetica</i>			

**Table 2: New European algebra textbooks 1600 - 1630**

The following table has been compiled with the aid of Rider 1982.

Author	Date	Titles	Imprint	Language
Reimarus Ursus	1601	1	Frankfurt-an-der-Oder	German
Cataldi	1602-20	9	Bologna	Italian
Faulhaber	1604-22	5	Ulm, Frankfurt, Augsburg	Latin (3) German (2)
Kurz	1604, 1619	2	Nuremberg, Leipzig	German
Henisch	1605, 1609	2	Augsburg	Latin
Rothe	1607	1	Nuremberg	German
Smyters	1609	1		Dutch
Tapp	1613	1	London	English
Cyriaque de Mangin	1615-23	4	Paris	French
Lantz	1616	1	Colonia Aggripina	Latin
Sesen	1617	1	Hamburg	Latin
Brasser	1619	1	Hamburg	Latin
Graffenriedt	1619	1	Bern	German
Tolra	1619	1	Tarragon	Spanish
Ventallol	1619	1	Tarragon	Spanish
Mennher	1620	1	Paris	French
Lauremberg	1621	1	Hamburg	Latin
Follinus	1622	1	Cologne	Latin
Wilken	1622	1	Emden	Latin
Lauremberg	1623	1	Hamburg	Latin
Glorioso	1627	1	Naples	Latin
Schagen	1628	1	Rotterdam	Dutch
Girard	1629	1	Amsterdam	French
Roselen	1629	1	Cologne	German

### Summary

	Authors	Titles	Imprints
German	13	8	17
Dutch	3	2	3
Italian	2	9	10
French	3	6	5
Spanish	2	2	2
English	1	1	1
Latin		13	
Other			3
Total	24	41	41



**Table 3: Algebra texts published in England up to 1702**

Date	Author	Title	Reprinted
1557	Robert Recorde	<i>The whetstone of witte</i>	
1579	Thomas Digges	<i>Stratoticos</i>	1590
1596	William Phillip (translator)	<i>The pathway to knowledge</i>	
1613	John Tapp	<i>The pathway to knowledge</i>	
1631	Thomas Harriot	<i>Artis analyticae praxis</i>	
1631	William Oughtred	<i>Clavis mathematicae</i>	1647, 1648, 1652, 1667, 1693, 1694, 1698, 1702
1650	Richard Balam	<i>Algebra, or the doctrine of composing, inferring and resolving equations</i>	1653
1650	Jonas Moore	<i>Arithmetick</i>	
1655	Thomas Gibson	<i>Syntaxis mathematica</i>	
1659	Samuel Foster John Twysden	<i>Miscellanies, or mathematical lucubrations</i>	
1664	Michael Dary	<i>The general doctrine of equation</i>	1669
1668	Johannes Rahn John Pell	<i>An introduction to algebra</i>	
1673	John Kersey	<i>The elements of that mathematical art commonly called algebra</i>	
1681	Jonas Moore	<i>A new system of the mathematicks</i>	
1682	Gilbert Clark	<i>Oughtredus explicatus</i>	
1684	Thomas Baker	<i>The geometrical key: or the gate of equations unlocked</i>	
1685	John Wallis	<i>A treatise of algebra both historical and practical</i>	1693
1690	William Leybourn	<i>Cursus mathematicus</i>	
1690	Joseph Raphson	<i>Analysis aequationum universalis</i>	1697, 1702
1693	Joannes Alexander	<i>Synopsis algebraica</i>	
1694	Richard Sault	<i>Treatise of algebra</i>	
1695	John Ward	<i>A compendium of algebra containing plain, easy and concise rules in that mysterious science</i>	1698
1700	Johann Sturm	<i>Mathesis enucleata</i>	
1702	John Harris	<i>A new short treatise of algebra</i>	

**Table 4: Editions and contents of Oughtred's *Clavis***

Edition	1 <sup>st</sup> Latin	1 <sup>st</sup> English	2 <sup>nd</sup> Latin	3 <sup>rd</sup> Latin	4 <sup>th</sup> Latin	5 <sup>th</sup> Latin	2 <sup>nd</sup> English
Imprint	London 1631	London 1647	London 1648	Oxford 1652	Oxford 1667	Oxford 1693, 1698	Oxford 1694, 1702
Title	<i>Arithmeticae in numeris et speciebus</i>	<i>The key of the mathematics</i>	<i>Clavis mathematicae</i>	<i>Clavis mathematicae</i>	<i>Clavis mathematicae</i>	<i>Clavis mathematicae</i>	<i>Key of the mathematicks</i>
Preface	1 <sup>st</sup>	2 <sup>nd</sup>	None	3 <sup>rd</sup>	as 1652	as 1652	none
Chapters	1 Notation 2-5 Four operations 6 Proportion 7 HCF 8-10 Fractions 11 Z,X,P,R/S 12-13 Powers 14 Roots 15 Surds 16 Equations 17 Examples of ch 11 18 Examples of ch 12 19 Euclid II.1-14 20 Problems	6 enlarged  11 more notation  16 enlarged  17 as ch 18 of 1631 18 Euclid II.5-10 Circles Theorems and problems 19 Euclid II.10-14 APs Problems	as 1647	as 1647	as 1647	as 1647	as 1647
Appendices		Equations Interest False position Sun dials	Equations Interest False position Euclid X Solids	Equations Euclid X Solids Interest False position Archimedes Sundials	as 1652	as 1652	as 1652

Table 5: A comparison between Harriot's manuscripts, Torporley's copy and the *Praxis*.

British Library Add MS (Harriot)	Harriot's title	Torporley's title	Sion college Arc MS L.40.2/L.40 (Torporley)	Praxis
6784 ff. 322-325 (a)	<i>Operationes logisticae in notis</i>	<i>Operationes logisticae in notis</i>	ff. 35-35 <sup>v</sup> (a)	Part I Section 1
6783 ff. 1-11 (b)	<i>De radicalibus</i>	<i>De radicalibus</i>	ff. 35 <sup>v</sup> -41 <sup>v</sup> (b)	-
6782 ff. 388-417 (c)	<i>De numerosa potestatum resolutione</i>	<i>De numerosa potestatum resolutione</i>	ff. 49 <sup>v</sup> -52 <sup>v</sup>	Part II <i>Exegetice numerosa</i>
6783 ff. 163-183 (d)	<i>De generatione aequationum canonicarum</i>	<i>De generatione aequationum canonicarum</i>	ff. 41 <sup>v</sup> -43 <sup>v</sup> (d)	Part I Sections 2,3,4
6783 ff. 98-112 (e)	<i>De resolutione aequationum per reductionem</i>	[systematic treatment of cubics]	ff. 44-46 <sup>v</sup> (e)	Part I Sections 5,6
6783 ff. 113-162 (f)	<i>De resolutione aequationum per reductionem</i>	[systematic treatment of quartics]	ff. 46 <sup>v</sup> -49 <sup>v</sup> (f.a-?)	Part I section 6
6782 ff. 107-146 <sup>v</sup>	at start: <i>De numeris triangularibus et de inde De Progressionibus Arithmeticis</i> <i>Magisteria Magna TH</i> at end: <i>Thomae Harrioti magisteria numerorum triangularium et inde progressionum arithmeticarum</i>	<i>T<sup>o</sup> H<sup>i</sup> magisteria ... triangulorem et de prog<sup>n</sup> Arithmet<sup>m</sup>;</i> <i>De NS Tr<sup>s</sup> et De Prog<sup>n</sup> Arith<sup>m</sup> Magisteria magna TH</i> [headings only, Harriot's order reversed]	f. 54 <sup>v</sup>	-
6782 f. 147-159	<i>Ad numeros triangulos quadratos pentagonos etc et illorum progressioness</i>	<i>Numeros triangulos quadratos pentagos et illorum progenes</i>	f. 54	-
from 6782 ff. 33-41	<i>Of combinations</i>	<i>Of combinations</i>	f. 54-54 <sup>v</sup>	-



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